

THE PSYCHOLOGICAL REVIEW.

THE PSYCHOLOGY OF INVENTION.¹

BY PROFESSOR JOSIAH ROYCE.

Harvard University.

In compliance with the kind request of our President, I have consented to open this discussion; but I do so feeling that the subject is not one which I myself should have chosen to discuss. Its importance I recognize, as we all do; but its difficulty is notorious. In the present state of our knowledge of the processes involved I do not think that it is possible to attempt much more than a statement of problems, and some indication of those methods of work by which in future we may obtain more light. With this understanding—that I am here not to state results, but to assist, however little, in opening, or at least in suggesting, some lines of inquiry—I accept the proposed task, while I am fully aware of the very modest character of the little contribution that I can make. This contribution will consist: (1) of a definition of the problem, (2) of a thesis as to certain pretty vague general conditions which favor inventiveness, and (3) of some merely illustrative experimental reports, intended not to prove but to make more comprehensible my thesis.

It is difficult of course to give any psychological definition of what is meant by invention. Ordinarily, if we take the word 'invention' in a decidedly wide sense, we mean by invention an important new idea, or system of ideas, or an important device or system of devices, by means of which such new ideas are expressed. The first way in which this general

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definition has to be called vague, is due to the fact that the word 'important' is not a term of description but of estimate. When is a new idea or a new device important enough to be called an invention? There is no scientific answer to this question. And the value of a given mental product is something with which the psychologist, who considers how that product came to pass, has only an indirect concern. The second respect in which this definition just suggested is necessarily vague has to do with the meaning of the word 'new.' In a strict sense, one may say that, while we may have new experiences to any extent you please, we cannot have absolutely new ideas, since an entirely new idea is a contradiction in terms. In the same way, every device, such as a word, a sentence, a poem, a mechanism, is inevitably based upon former devices, is composed of elements previously existing and well known, and is so far never entirely new. On the other hand, it is also true that in all mental life there is an element of novelty. Every moment of our mental existence differs in some respects from any previous moment of our lives. And about our actions and expressions of our ideas there are similar elements of relative novelty, however slight this novelty may be. When, then, shall an idea or an invention be called new? Still a third element of vagueness enters into the foregoing definition, in so far as, whenever we speak of a new idea or device, the question necessarily arises: *For whom* is this device or idea new? If I myself think a given thought for the first time, if I myself produce a contrivance that I never produced before, the act involves for me what is relatively an invention. On the other hand, with reference to the race at large, or to the thoughts and habits of the community in which I live, the idea or the contrivance may be very old. Inventions are, therefore, inventions either for the individual, or for the society of which he is a member. Ideas and devices are new in the life of this person only, or in the life of mankind in general; and one has to bear this distinction in mind in estimating novelty. So far then the importance and the novelty of any invention are matters only vaguely definable; and one has also to define for whom the invention is in any case to be a novelty.

If, in view of the difficulty of defining invention in all these respects, we try to consider afresh the situation in which our questions arise, a glance at well-known psychological considerations will enable us to characterize our problem more exactly. We cannot exactly define what is worthy to be called a valuable invention, but we can define some of the processes that condition inventiveness. The acts of any intelligent human being who has received training are, on the one side, expressions of the law of habit. That is to say, they are repetitions, more or less exact, of acts and systems of acts that have been performed before. Habits themselves are the results of adjustment to environment. The sense impressions to which an organism has been subjected, working upon the basis of its inherited tendencies, have led to the gradual moulding of the original instincts, impulses and reflexes of the organism in question; and the outcome of all this moulding is a system of habits which tends towards an invariable routine, although the routine actually attained by any individual organism is never during its normal life absolutely invariable. Some of these habits, namely the most complex and the most deliberate, are accompanied by consciousness; such habits we call intelligent. On the highest levels, the acts in which these habits get embodied are the expression of ideas or of trains of ideas, of thoughts, of states of mind which involve a knowledge of the environment. Now, in consequence of the general laws of habit, the most of what is done during the mature life of any intelligent being pretty closely resembles what has been done before. The habits expressed when we use language are, for instance, old habits. The words that we use have been used before. But under certain circumstances a change of stimulation may cause already acquired habits to vary in ways which involve new combinations of old but already intelligent activities—and in ways which do not merely repeat current facts of experience. These independent variations of intelligent habits constitute the general region within which are found the activities that we call invention; and the ideas or system of ideas that accompany these variations of intelligent habit are, on the mental side, more or less inventive psychoses. Thus a new combination of words may constitute a poem. This combination is not

wholly due to the present facts of the poet's outer experience. It is in so far an independent variation of his habits of speech. Thus all inventions are relatively independent variations of intelligent habits, and the psychological question as to the origin of inventions is parallel to the biological question as to the origin of variations. Meanwhile it remains true, as before, that not every such variation of intelligent habits, but only the important variations, are usually dignified by the title of invention. But the psychologist is more interested in the appearance of the variation than in its importance for the individual.

Thus one may view inventions in their relations to an individual life. If we turn from inventions, in so far as they are novelties in the individual's life, to the variations of habits that are novel in the life of society, we are met by a further situation which may be briefly summarized as follows: What the individuals in society mostly learn to do is to imitate other individuals. The intelligent adjustment of the social being to his environment involves conforming his ways to the ways of other people. As Professor Baldwin has pointed out, the law of imitation in the social order is a sort of extension of the law of habit in the individual life. In his habits the individual repeats himself. In his imitations, even when, as he learns them, they are for him novel, the individual repeats what society has already accomplished. For this reason most variations of individual habit in the social being, most inventions of an individual life, involve no variation of the habits of society, involve no essentially novel ideas or deeds. On the other hand, it is true that, despite this tendency to uniformity, there do appear in social life from time to time relatively original independent activities—deeds that have never been done before by anybody, combinations of ideas that in some respects are not due to imitation, that are not the results of the past habits of individuals simply repeated, or of the habits of society simply imitated. If these variations of our imitative activities themselves get imitations from others, they constitute, in the social sense, inventions. Viewed from the social point of view they then come to have the same relations to what men in general do, as in the individual life the relatively novel variations of habit have to the past deeds of the individual.

To sum up so far, we have thus two distinct processes worthy to be called inventive. Inventions involve intelligent variations of habits already acquired and present in the individual; and where inventions are socially important, they involve similarly independent variations of imitative activities, or variations of social habits. The question about the psychology of invention is therefore twofold. It is, first, Under what conditions does an individual tend to vary his own already established and intelligent habits? and, second, Under what conditions does the individual tend to be what is called original, namely, not imitative? The two questions are, of course, very closely connected.

The question of the variability of our individual habits, where the variations are of an intelligent grade, and accompany the appearance of relatively novel ideas or combinations of ideas, constitutes a very wide and difficult problem. What we know about it may be indicated by considerations which every psychologist will recognize. In the most limited degree it is true of every act that, because it is done under novel conditions of experience, it tends to involve some variation of former acts. It is because of this plasticity to experience that we originally acquire our habits at all. This primary sort of plasticity remains, to a certain extent, present even long after our habits have reached an intelligent grade. One lives and learns. But mere learning by experience, adjusting our ideas to presented facts, is rightly distinguished from true inventiveness, which involves novel ideas and acts that are not merely determined by outer experience. In the next place, the organs used in our more active life tend in an extremely complex way to acquire one the habits of another, so that unconsciously we all of us are in possession of a great number of habits of movement which have never been purposely acquired, and have never been merely adjusted to outer facts, but which have been imitated, so to speak, by one group of nerve centres in consequence of organic connections with another group. Thus, one can on occasion write with the foot or with the knee. The left hand is frequently able to repeat symmetrically the movements already learned by the right hand. Such unconscious co-education of our various organs is itself a source of considerable independent variability in our actions.

For a habit to which one group of organs has been adjusted becomes *ipso facto* altered when it is imitated in this unconscious way by another group of organs. And everybody has a great deal of skill of this unconscious sort which can be called out on occasion, and which can be adjusted to new tasks. Finally, those laws which psychologically appear as the laws of association of ideas involve, as every one knows, a good deal of relatively independent novelty both of ideas and of activities, whenever our organisms are brought into new situations.

Thus mental processes which Stout has called 'Relative Suggestion' involve a certain independent novelty in the suggested ideas. For instance, my habits may have already determined, on the mental side, the combination of two ideas, *A* and *B*, where *A* and *B* are contents standing in a given relation to one another. This habit is aroused, for any reason you please, in the presence of some new content, *P*; and the associative process may so work that I thereupon form a relatively new idea, *Q*, which stands in the same relation to *P* as that in which *B* had stood to *A*. Thus, in consequence of this sort of relative suggestion, *Q* appears in my mind as a novel mental content, not directly derived from experience. The mere form of the combination *A B* has been repeated, without the repetition of the matter. On the motor side this sort of permanence of form with alteration of content in the expressions of our habits is not unfamiliar, and appears in all our more intelligent actions. In this way we tend not merely to repeat literally our former acts, but to produce acts that have merely the same form or general type as former acts. Most of our intelligent habits are thus what one may call generalized habits. Such, for example, are the habits of the syntax of our mother tongue. These involve not merely the power to repeat old phrases, but the power to make new combinations of an old type. Such habits are essentially, within limits, variable habits. In a measure, they are habits which involve a certain novelty of behavior at every new expression of the habit. There is routine running through all this novelty. But it is the routine of form, not of content, or at least not altogether of content. Our social habits show endless variations of this tendency to permanence of form in conduct

amidst great variety of detail. Consider, for instance, the habit of repartee, the habit of courteously adapting one's behavior to the present social atmosphere, and many similar cases. Athletic habits involve much of the same sort of generalization. The skilful player may at any moment do something which is in content more or less different from anything that he ever did before; but his skill shows itself in the form. Now, in all these cases, where the nature of a habit is such that a given form is preserved through great variations of content, whether in ideas or in actions, the variability that appears is not itself an exception to the law of habit. On the contrary, in such cases the intelligent habits in question are, as I just said, essentially habits of variation, although the variation is in most such cases subordinate to the routine, and the range of variation is very sharply limited. Habits of this sort constitute precisely what we mean by skill. All skill involves, therefore, some more or less obvious, although limited degree of inventiveness.

In one further direction, however, we can understand the way in which our habits may slowly vary, and vary in useful directions. Professor Baldwin has laid great stress upon the influence of the 'try, try, try again' tendency in the early stages of formation of habit. At any stage of our development we are possessed of an imperfect adjustment. We elaborately repeat that adjustment in constantly altering situations, with a steady disposition to eliminate any useless elements. Here the constantly changing situation involves a constant slight alteration of what we do. A steady selection of the slight variations leads to improvement of habit.

To sum up this brief survey of the factors in the individual variation of habit, we see that in all their routine our habits have in normal cases a considerable tendency to independent variation, apart from our direct dependence on the facts of outer experience, although this variation does not in general tend to produce very great or very important alterations of behavior, except during the formative periods in the life of an organism. The defect of all such considerations, if regarded as an explanation of the variations which we call, in the narrower sense, significant or true inventions, lies in the fact that all these tenden-

cies are in general useful in so far as they lead *towards* routine, and tend to make both our life and our knowledge systematized, and in the end simplified. The great inventions of humanity all seem to include processes more complex, and more mysteriously rational than this ordinary routine of variability will explain. Skill is not talent. The artisan is not the artist. The apt scholar is not yet the discoverer.

If we pass to the relations between the individual and society, we must admit that just as all our habits within limits normally tend to vary, so all the imitative social processes normally involve certain individual variations. Every imitator is, in his own little way, an originator. For every organism inevitably colors its imitations with its own individual qualities. Our handwritings are different, even where we have been taught to imitate the same models. Our voices are individually recognizable in their variations, even when we sing the same note, or repeat the same words. The imitative life is in so far also an individual life. And every individual is thus a possible source of socially important variations. Chance may make anybody a socially prominent person, just as Captain Boycott's name but a few years since entered, perhaps forever, the English Dictionary. Moreover, since no two individuals can have precisely the same social environment, since the range of acquaintances, not to speak of closer affections, is inevitably an individual range, no two individuals have the same models to imitate. Where two individuals are trying to imitate the models or fashions of conduct presented in the life of the same social group, every individual has more acts suggested to him than he can possibly succeed in carrying out, and, therefore, each of our two individuals is obliged to select for himself what models he proposes to imitate. The social order is a sort of elective system of social instruction, where every individual to a certain extent chooses his own models. Hence, despite the vast power of social routine, despite the universal prevalence of imitation, there is always, even in the life of a savage tribe, a great deal in the social order that constantly tends to favor social variation. Yet this variation is confined within comparatively narrow limits, except in the case of the most progressive societies, and of the most plastic individuals.

If we now put together, after our brief survey of the known field, the social and the individual factors that tend to favor variation of habit and of idea, we thus find that while there are important factors tending to work against the too literal repetition of habits and of ideas, it is hard to point out precisely the conditions that favor those rapid and significant changes in the routine of action and of intelligence which appear when inventions of greater importance take place. And so, since there is often a wider range of variation favored and explained by decidedly pathological conditions, it becomes natural enough that some psychologists should have looked to pathological explanation as sufficient for all the forms of large variation, whether useful or useless. Hence the general basis in fact for the now so frequently current view that all great inventions and inventors are more or less pathological phenomena. Yet I do not believe that we are limited to such pathological explanations. In any case, by this survey of the known conditions, we reach the statement of our problem itself, which is this: What factors tend to produce such variations of habit, either in the individual life or in the activities of society, as more specially include the significant variations, the valuable novelties, that we call invention? Are such more extreme variations simply special instances of the ordinary processes that, as we have seen, tend to a constant, minute and relatively insignificant variation of our routine? Are such processes, on the other hand, due to wholly chance interferences with the normal laws of habit, to interferences of no definable type whatever? And is the problem of invention simply the problem as to how the useful variations get selected when in this wholly atypical way they happen to occur? Or can we define beforehand the conditions under which valuable variation is likely to take place?

As I said at the outset, it is rather the statement of our problem than its solution that I can hope here to attempt. Yet at this point *one* consideration occurs to me as worthy of notice. It is a consideration suggested by the history of invention. Important inventions do not, in general, occur except under particular social conditions. And the social conditions have their definable type. What is this type? In the individual the

most important independent variations of his habits occur during the growth of his social sense. The mere organic growth of the brain has, of course, a good deal to do with this youthful variability. But there can also be no doubt that it is the social sensitiveness of the young which is one very important factor in the same process. On the other hand, if we pass to mature minds, there cannot be the least doubt that individuals themselves vary more in their own habits, become more productive of novel processes, and contribute more to the variation of social habits, when the conditions are such as to favor the social tendencies often called by the general name individualism. And individualism means a mass of social tendencies having a definable type. In other words, the individual varies more in the long run when the society in which he belongs expects him to vary more, when variation is encouraged, when independence, private enterprise, is favored by the social environment. This is a very simple consideration, and very easily verified in history, as well as in individual psychology. It is a consideration which the psychologist cannot leave out of account. Children in the country, or children brought up in comparative isolation from school routine, often show a much greater inventiveness in their games and romances than do children early submitted to the routine of large schools. Anybody who has watched this process in a relatively isolated child recognizes at once that the child is still dependent upon his social environment for his ideas, and that also the relative independence of his situation favors the variation of his habits. In society the same thing holds. The periods of great individualism have been periods of relatively great inventiveness. This was the case in Athens at the great period. It was the case in the Renaissance. It was the case during the Revolutionary period at the close of the last century and the beginning of this. It is the case wherever, in a highly intelligent people, similar conditions have prevailed. Nor can the processes present at such periods be referred simply to the happy chance that geniuses were then produced. To be sure, I have no idea of explaining the greatest cases of genius, or of reducing them to any one law. I am here referring to the average inventiveness of

the really clever men who, in the civilized races, are constantly produced. In the great age of individualism, the lesser men invent as well as the greatest. On the other hand, the uninventive ages in the history of civilized people are characterized, not by the mere absence of geniuses, but by the helplessness of the men of talent to accomplish anything of importance. Now, what is known of the biological conditions of heredity would make it very improbable that, extraordinary genius apart, the organic basis for the variation of talent, for the appearance of milder forms of originality, should be very different in one age, in a given stock of people, from what it is in another age in the life of the same people. But, in the life of any civilized stock there are periods of invention, and periods of stagnation, and it seems to me that we may say, of society, that if we here deliberately leave out the cases of the greatest geniuses, civilized society, while dependent upon biological processes for the production of its men of talent, still gets out of these men of talent, in any age, very much what it deserves to get—*i. e.*, what in a proper way it asks for. It cannot produce the great genius, and it cannot make stupid men clever, but from its lesser men who are still men of real ability, it gets within limits very much the degree and the type of inventiveness that the social situation suggests. In a poetical age, poetry is invented by the second-rate poets; and some of it is very good poetry. In a scientific age, scientific discovery is the order of the day, and the men of talent are scientific inventors. During a period of war, military ability is encouraged. Of course, no such social encouragement can produce Shakespeare's plays, or Darwin's discoveries, or Napoleon's achievements. And only heredity can account for the very wide differences between clever men and stupid men, or explain why men of talent exist at all. But the minor and still important inventiveness of the men of talent, the men of the second grade, is somehow due to a social stimulation which sets their habits varying in different directions. And this stimulation is of the type which abounds in periods of individualism. So much for a very obvious and general suggestion, which of course once more helps us to state rather than to solve our problem. I expect, of course, the immediate question, How can

one's social situation tend to make one more inventive, more variable in one's significant habits, than one otherwise would be? For, once more, the primary character of the social influences to which we are exposed is that, within limits, they set us to imitating models, they tend to make us creatures of social routine, slaves of the mob, or obedient servants of the world about us. On the other hand, if all that society says to a man at a given time is, Be inventive, be original, it seems indeed a very serious problem how the suggestion can possibly work; for if a man is left to his own devices, and tries to invent something of importance that is novel, he so often finds himself merely repeating old habits, or merely imitating models, that he often concludes that his inventiveness cannot be stimulated by any conscious suggestions whatever.

Inventions thus seem to be the results of the encouragement of individuality. Yet how individuality can be encouraged to go beyond its limits is a very serious problem. Here then is a new statement of our problem. The problem of the psychology of invention in the more important social cases becomes the problem of the psychology of the tendency called individualism. What sort of influence is it that puts the individual on his mettle, that awakens him to valuable and independent variability of habit, that, as they say, makes him let himself go? The problem is familiar in pedagogy. But can we suggest any new way of illustrating it when we approach it from the side of the psychology of invention?

In thinking over this problem, I have of course tried to inquire what form of experiment could be devised for the encouragement, in however slight a form, of something dimly resembling individuality and inventiveness. Inventions, I suppose, can be experimentally produced in the laboratory in some miniature shape. The miniature might indicate the nature of the great fact, and so I cannot forbear to bring before you the results, such as they are, of a few very insignificant efforts to produce a situation where the subject of an experiment should be encouraged to invent something. The small value that such experiments can have lies in the very simplicity of the conditions used, and in the fact that, in a very small miniature, such con-

ditions may be made to simulate the motives that, in societies, seem connected with individualism.

It occurred to me to choose in a number of subjects a certain variable group of habits, and to submit this group to specific experiments. The habits chosen were to be not wholly unintelligent. On the other hand, they were to be habits not already too much subject to social training, or to reflective observation on the subject's own part. Furthermore, they were to be habits that could be exposed, first to the workings of the private inventiveness of the subject himself, and, secondly, to the workings of a distinctly social stimulation—a stimulation of the same sort that exists when in a company of people we are urged to do our best, or are put on our mettle. My object was to get some glimmering of the way in which such a social stimulation becomes effective. In order to get my case simple enough to be of any value whatever, I had to put the subject in the dark as to the purpose of the experiment, and to make the social encouragement introduced of a very mild and minute type so that I could regard it as a factor somewhat isolated from other factors in the conduct of the individual experiment. What I actually did—or rather began to do, for the brief time that has elapsed since I was asked to make this report has been too short to admit of any extended series of experiments—was this : Taking subjects in groups not too large to be controlled, I made each subject perform three, or in some experiments four, series of acts according to directions. In the completer experiments, where four series of acts were tried, the method was that, first, the subject was asked to draw on ten cards, one after another, and as quickly as possible, some figure or combination of curves and straight lines, which should not be an imitation of anything, so far as he could keep himself from such imitation. He was asked to throw aside each card as he drew it, and not to look back. He was asked to make his design each time as independent of former designs as possible. He was not to erase anything that he drew. He was to make each design at one movement. The plan of the first series having been carefully explained and understood, the subject, who was not to design until the experiment began, began at a signal, worked as fast as he could, and

was required to finish the ten designs within two minutes. In the second series of ten, the subject was required to continue drawing new designs that imitated nothing and that were independent each time. "Draw something new each time," I said, "but this time be deliberate. Do what you do as carefully as possible, only throw aside each card as soon as it is done." This second series completed my test of the subject's independent inventiveness; that is, of his inventiveness apart from social stimulation.

Now, for the second half of my experimental test I wanted, as I said, to get a stimulus of the sort to put the man on his mettle, and one that still does not permit him to be satisfied with what he takes to be an imitation. In reading the history of inventions, and in observing in general the inventiveness of children, I have been much struck with the effectiveness in exciting originality of a certain motive which I may call the motive of being in a decidedly sharp contrast with one's social environment. This I should call *one* typical motive of all individualism. The child that desires to show himself off, the successful wit, the adept at repartee, the ambitious young poet, and in general a man of mark in an age of great individualism, all illustrate the psychological effectiveness, within certain limits, of the mere desire to make a contrast. A contrast of this sort is at first a vague ideal. It is, however, an ideal that tends to grow definite as it works. At first an illogical motive, it tends to grow more logical as it is applied. For the dwelling upon a contrast, the mere effort to show one's skill by reducing the contrast to some deeper sort of similarity, the studious effort to invent something that shall at once take account of the existing contrast, emphasize it, and, at the same time, reduce it to some sort of deeper uniformity with its opposites, this has been a motive even in the pursuit of the soberest science. Such a motive led, for instance, to Plato's philosophy, or to the mathematical concepts of zero and of negative quantities. Before I ventured on the experimental suggestion of the second half of the experiment, I accordingly said to the subject: I am now going to show you in succession ten cards drawn at random, just as you have been drawing yours. As your third series, I

want you, on the sight of each card, to draw at once and without the least reflection some object that feels to you at the moment when you draw it like a new design, but that *also feels as unlike as possible to the object that you see*. I added the observation that the subject must find out for himself in each case what 'unlike' meant; that I could not tell him beforehand; and that I simply wanted him to draw as well as he could. It was simply this stimulus of the unlike, this *Geist der stets verneint*, which constitutes the Mephistopheles that I wanted for stirring up my subjects;¹ and I suppose that we shall all agree as to the interest of any attempt to get the devil to assist in a bit of experimental psychology. After ten such cards had been drawn, I then let the subject at his leisure compare these cards one after another with the model, deliberately consider whether he had made them as unlike as possible, and draw a fourth series of cards containing if possible new unlikenesses and unlikenesses as great as possible. In some of the earlier experiments, for fear of wearying the subject, I at first used only three series of experiments, omitting what later became the second set, namely the deliberate efforts at unaided invention. But in most of the experiments four sets were used.²

Throughout the experiment great care was taken to give all the subjects concerned the same directions, and to make the inevitable suggestions involved in these directions as uniform as possible. Thus, I had to make clear that the designs were not to be imitative of any object. In making this statement I used always the same names of objects, or rather of classes of objects, to indicate the nature of this exclusion. I said, 'no character of an alphabet, no picture of an object,' and so forth, using as nearly as possible the same formula, although I often had to repeat or slightly to vary my phrase in order to make the matter perfectly

¹ Des Menschen Thätigkeit kann allzu leicht erschlafen
Drum geb' ich gern ihm den Gesellen zu,
Der reizt und wirkt und muss als Teufel, schaffen.

² I must explain, to avoid an obvious misunderstanding, that I do *not* suppose any true poets, or other inventors, to be guided by so gross a motive as the one here used, viz., by the motive to be merely *as unlike as possible* to their predecessors. I have wished to isolate the motive explained in the text. Hence the grossly abstract form here given to this motive.

clear to each subject before beginning. In presenting the objects that were to serve as the stimulation of the unlike I used a set of drawings very much resembling in their random type the sorts of drawings that I expected to get from the experiment. I used the same set of presented drawings throughout. In one case the drawings were presented in a very slightly altered order. But as direct suggestion from the particular drawing presented played a decidedly minor part in any single resulting drawing, this one change of order seems to have been of small importance. Yet I should of course avoid it in any final preparation for drawing detailed conclusions from further experiments. In the fourth column experiments of the charts presented, the subjects commonly had the opportunity to see more than one of the drawings at the time, but as they had by this time seen them all once and were now engaged in a fixed effort of attention comparing their own work with the particular drawing, I did not deem it necessary at this stage to guard against such manifold suggestions. In general, as I just said, direct suggestions from the drawings shown played a minor part in the results. The experiments test, therefore, in a rough way, the results of one interference nearly constant in all cases.

Now I need not say that it was far from my thought to obtain from such experiments any final or exact results whatever. I regarded them, and regard them, merely as giving a first glimpse into the labyrinth of the influence of social suggestion upon individual inventiveness. I should be in such a case well satisfied to find that a stimulus of this sort gave results that made it seem in any sense an encouragement of individuality. I can best deal with the experiments and their suggestiveness by presenting to you the enlarged charts of twelve results.¹

¹Of the charts here presented, the first three, and the twelfth, are reproduced in the figures numbered 1, 2, 3 and 4. The figures given are intended to show in brief rather the way in which the cases varied, than the degree of inventiveness actually to be found. Of the latter only a statistical estimate could be of service. A preliminary, but pretty careful study of 40 cases, the work of people of both sexes, and of widely varying ages and occupations, showed 24 out of the 40 as cases where in the later columns, as compared with the earlier, a decided *increase* of inventiveness could be observed. I give this not as a result, but as a mere beginning.

An examination of these charts as to their general appearance will first indicate that they are, to say the least, suggestive of a possible method for the study of individual psychology. The first ten, the independent drawings, have a very decidedly individual range of variation and uniformity. Each person challenged to draw ten designs of this sort, will draw a set of designs that obviously stand for a system of habits, some of them no doubt technical and conscious at some time in the past, or even at present—some of them, however, especially in people not accustomed to drawing, habits more or less subconscious. They vary from individual to individual in a way that is certainly worth observing. I suppose that the particular experiment taps but a very small portion of the world of motor-habits here involved. The second column's work in the four-column cases in our charts would stand of course for the second ten, namely, the results of deliberate individual effort to vary the designs. These efforts, of course, made thus hastily, cannot be regarded as just to the individual's actual range of power to construct such designs. But they do illustrate what habits of the individual belonging to this range were just then at control under the conditions of this experiment. Now comes the stimulus in the form of the requirement: See that, and make the unlike. What will the individual do under such circumstances? Will he helplessly follow the suggestions of the models presented, and make something like them in trying to make the unlike? Will he do what he might well do, namely, simply continue the style of his private inventions of this type, without variation of style? If he did this latter, he would in general make something decidedly unlike the models shown. Will he start upon an entirely new track? Will he be frightened out of his inventiveness altogether and be unable to make any designs? Or will he, finally, be stimulated to become more inventive, whether by combining his own style with the presented styles or by striking off on entirely original lines? Very naturally, I found subjects who did nearly every one of these things. The actual range of variation in some eighty cases¹ where the experiment was tried, is consider-

¹ Out of these eighty cases were chosen, at random, the forty that were submitted to the more careful statistical study mentioned in the previous note.

ably greater than in the charts here displayed. I took, however, some of the most striking results for consideration. And I may ask you at this point to form your own opinion on the cases presented.

The subjects of the experiments vary greatly in training and in age. I begin with the case of a workingman, aged 47, a stitcher by trade, a member of a workingmen's club, and performing the experiments with undoubted good-will. He had no warning of the nature of the work and is of course without psychological training. You see (Fig. 1) that he begins his independent designs with very short curved marks, without apparent ideas of any objects, and with variation merely in the direction of this or that small feature of the curves used. His second column—that of his deliberate but still uninfluenced invention—involves making small objects of an indefinite character. The rather rough method of reproduction here used—namely a pantagraph pencil marking marked over with crayon—is somewhat unjust to the smaller outlines of his drawings. The last one tended in the original to look somewhat like a glove. All are small objects with closed outlines; a rounded form predominates. The type is one that could not very widely vary. The last figure suggests that the subject could not go further without drawing genuine objects. With the third column began the successive display of the cards which were to stimulate the subject to make the 'unlike.' Notice at once the very marked change. At once his entire style altered. The first alteration is, as it chanced, a distinct suggestion from the model. This character remains, although not in a very marked way, in later cases. It soon becomes hard to say in what fashion the subject was influenced by the individual object seen; or at all events, where this suggestion can be traced it is usually of a rather general character. But mark, whether you call it individual suggestion or not, there can be no doubt our subject is trying to make his

The value of the estimates made can be, in a measure, tested by comparing the text and the plates in the four cases here reproduced. More careful statistical studies, with a fuller statement of the methods of estimate used, will, I hope, be given hereafter. It has not been possible to reproduce here in plates the drawings used as the stimulus for the 'unlike.' They were a set of rude outlines, of the general character here in question.

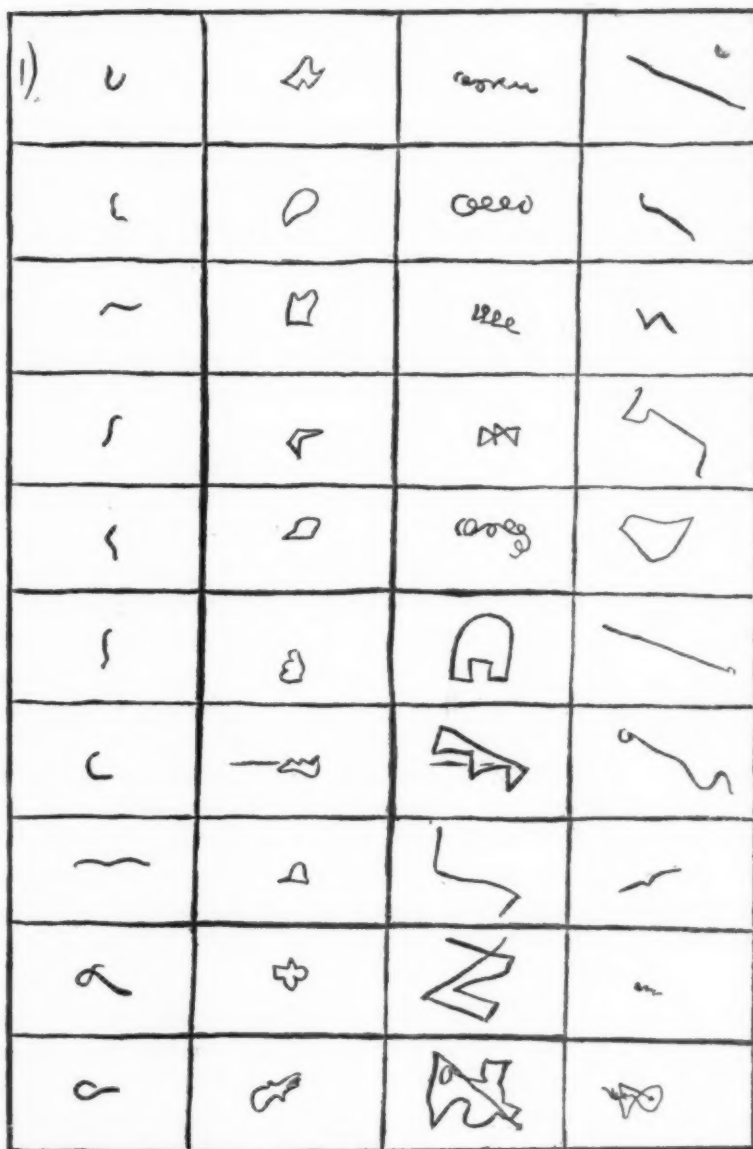


FIG. 1.

objects unlike; and there can be no doubt that in so doing his inventiveness is increased. It is noticeable that all these cases were made very rapidly, within a little more than two minutes

for the whole ten. The cards were seen and taken down at once. The fourth column shows still a new character. The subjects return to simplification. He is now deliberate. He is not so inventive in novel forms. In his case the stimulus works best as the cause of variation in his habits and association, or if you like, in his selections from amongst his habits, when the stimulus works quickly. In any case, there can be no doubt of the actual importance of the new stimulus as a cause of variation.

A notable contrast exists between the foregoing case and No. 2. Here the subject is a young lady, an elementary student of psychology, with a reputation as a clever and successful student. Like the foregoing subject, this subject disclaims any skill in drawing. In the first column are independent designs drawn without deliberation. The subject was given in this case no opportunity to develop her independent inventiveness. Doubtless, if left to herself, she could have made her scroll-work considerably more complex. But the habit is a highly developed one. When questioned, the subject declared that it had long existed, and was a subconscious habit—namely, the habit of making symmetrical curved forms for amusement. Technical skill was not reported in this case as being in any way responsible for the forms. In the second column the intruder appeared. Here a rapid and important change occurs. For the moment the subject is, as it were, 'all broken up' by the requirement to make the unlike and to make it quickly. The formless scrolls resulting are of course not genuine novelties, but do represent, apparently, either a reversion to the very early habits or a disintegration of existing ones. Whatever the effect, it is only momentary, although the effect of suggestion from the cards shown is visible and yet productive of no valuable result. With the fourth case the subject begins to return to her own style. Her scroll-work habit rises again to the surface. It is now more elaborate than ever, although no more deliberation is allowed. The slighter influences of suggestion from the models are not invisible. They act to produce more varied forms, which are, perhaps, not less worthy of regard. But when, in the third column, the subject is given a chance to deliberate, she produces in the first, third and fourth cases forms

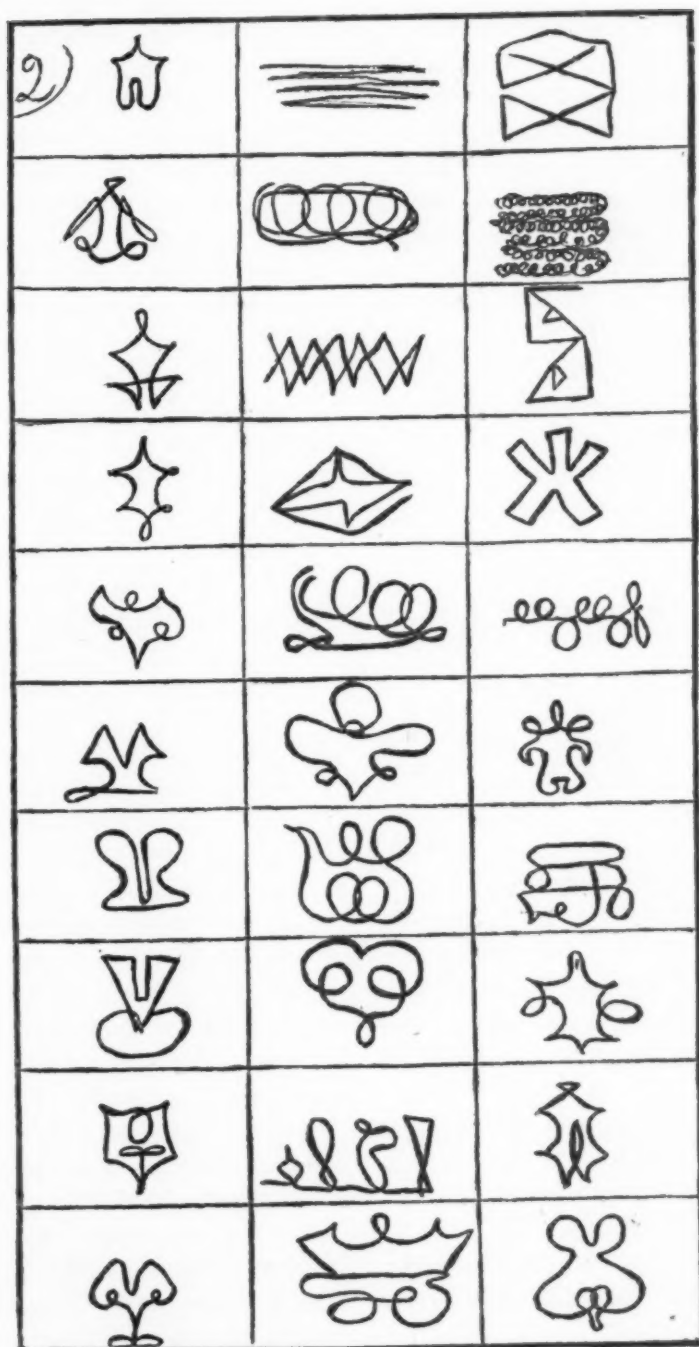


FIG. 2.

which are useful combinations of her own bias for symmetry with suggestions due no doubt to the objects seen. In notes which the subject independently made upon the latter cases of this third column, notes made while she drew the deliberate forms, she explained, with a certain *naïveté*, that, since the figures which she saw were in these cases unsymmetrical, she might as well oppose bi-lateral symmetry to them as her own invention of the unlike. She was plainly unconscious at the moment of the prevalence of such symmetry in her now unseen drawings of the first group. Afterwards she said she recognized the permanence and depth of the subconscious habits involved.

This case introduces us to a type present with the most manifold variations in numerous cases amongst our experiments. I should venture to call this the self-preservative type. It is notable that the forms produced in our first column would in general serve well enough to fulfill the vague requirement of the 'as unlike as possible.' The subject whom we first considered was unable to make use of the forms of the first column when the intruder first appeared. His 'unlike' had to be a new invention. In the present case, after the momentary scattering of the subject's habits by the intruder, the subject returned to the type of the first column. But it is notable that in returning to this type she returns to it in an enriched and more variable form. She preserves her own fashion, but with an addition which leads in two or three cases of the last column to what one might call a genuine novelty, namely, a new type of symmetrical figure.

Our third subject is a salesman, aged 41. He is an interesting modification of the self-preservative type. He begins (Fig. 3) with short lines; habits of writing are evidently involved; letters appear, although he tries to escape from them. The process, left to itself, turns into a making of flourishes. These are combined on two plans, one the plan of a spiral in and in interweaving, the other the plan of overlaying one flourish by another. With the appearance of the intruder a slight change occurs in these habits. The flourishes get a little more of the zigzag effect. They are, at the same time, more open. No great elaboration occurs. But, in general, whether with or without deliberation, the effort to make the unlike results in a pretty constant and subtle mod-

It is just such blending that, in some arts and even in some sort of scientific work, constitutes valuable inventiveness.

Decidedly different is the fourth case. A young man, in the third year of psychology work, is here in question. The cases of the first column are the quick-drawn ones. They are all small objects, suggestive of natural history studies. They have closed outlines, and strive to be something. The intruder introduces a number of new shapes, makes the figures larger, but does not in general alter their character of closed outlines, or their tendency to resemble natural history objects. The type is self-preservative. Some general suggestions from the objects shown are visible. The result is that the subject improves, if anything, the expressiveness and the character of the habits which he originally employed.

Plain so far is the fact that the requirement as made for the first column is a requirement that does appeal to pretty definite individual habits, that vary in certain persons. Notable also is the fact that the intruder produces a change in these habits which is in part, as you would expect, a reflex, a combination of dim suggestions, with existing tendencies, and which, whether or no it decidedly alters the first habits, at least tends to make the conditions of the work more varying, and possibly more inventive. But what is the nature of the influence of the intruder? If the cards shown were to be imitated, we know perfectly well what the influence would be. The subject would try to imitate, and within his limits and his own style he would do so. And in so far his habits would not tend to vary in any ways except those determined by the models shown. But our subjects are in an inventive position. Without knowing that they are to be specifically judged for their inventions, and without regarding this as any particular test of skill, since they, of course, find this business of making nothing in particular a business that gives little chance for skill, my subjects are still trying to vary their habits. They do so in new ways when the intruding stimulus intervenes. Is this because of the direct combination of the possibly suggested imitations of the models with their own habits? I should answer that the result of the stimulus is somewhat more complex than this. Compare, for instance, the following case :

The subject is here a salesman, aged 37. He is ready with his forms in the first column. These are obviously derived from handwriting; but he keeps fairly clear from complete letters. In the second column he introduces the motive of symmetry, and in so far grows in inventiveness. Now, why does he not continue just this process in the third column? On the contrary, he becomes at once, as it were, monosyllabic in his answer to the intruder's presence, like a person who stiffens or becomes relatively silent in the presence of an embarrassing social situation. Yet the forms chosen actually vary. One may well question whether the selection used is not about as great in the third column as in the first. The effects of direct suggestion from the model are hardly visible. In the fourth column the subject returns, as it were, from his relatively silent condition. Upon deliberation he has something to say to the intruder, but what he has to say is, on the whole, decidedly different in style, both from the style of the first column and from the style of the third. He has progressed to a new kind of invention. The type is not self-preservative, but it is the style of a man who, after all, meets the new requirement with at least some relatively new device.

Our next case is a bookbinder, aged 40. His style in the first column is very individual, and unlike what we have seen before. I suppose that the words that I used in describing the experiments, where I spoke of combining curves and angles, had to do with the choice here, but the style is as individual as a man's handwriting. Notice the entire change at the third column. The intruding stimulus entirely banishes the devices of the second column, although these have been very deliberately elaborated; they vanish not to return. More regular flourishes later take their place. But in the earlier cases of the second column you have a pretty logically varied selection of types differing from one another in definite geometrical fashion. The call 'make the unlike' here tends to arouse a systematic group of logically contrasted habits.

Our seventh case belongs once more to a relatively self-preservative type; and shows how the intruding stimulus, appearing in the midst of a lot of scroll-work patterns, at first scatters these, produces the small figure, monosyllabic type of reply to

the intruder—a reply as unlike as possible by being small when the intruder was big—while when the freedom of construction returns upon deliberation it returns in the forms of the columns 1 and 2, with the inventiveness as it were chastened, unable to run as freely as before.

Our eighth case is a three-column case, the work of a trained psychologist, and student of general science, an extremely critical and cautious man. He is distinctly less inventive in his second column than in his first. He knows why. It was because the intruder made him cool, considerate, critical, unwilling to do anything but precisely what was asked for.

The ninth case is again of the self-preservative type, although the intruding stimulus produces at first a large change. This subject found it almost impossible to avoid drawing objects. He was a man aged 20 who described himself as a private man and messenger. He was unable to avoid drawing what he had seen. The intruding stimulus first seemed to teach him to invent forms that were not objects, and certainly in so far increased the variability of his habits. Yet the tendency to return to objects was unconquerable. The result of the struggle between the new and the old is a decidedly large variation of style.

If, in the foregoing case, we had the relatively self-preservative quality, with the result that the habits involved varied in consequence of the struggle with the intruder, in case ten we have an illustration of the absolutely self-preservative type that can be taught, so to speak, almost nothing by the particular device used in this experiment. I need not say that I draw no conclusion as to the variability of the other habits of this subject. I report the case, partly to give a fair notion of how variable these cases themselves are, when compared with one another, and partly to suggest the use of this sort of experiment for individual psychology. I do not think that the subject knows the origin of these forms. She is a young lady in the first year of psychological study.

Case eleven illustrates in a happier way how self-preservation may be combined with invention. The subject is a woman, an advanced student of both psychology and general philosophy. The type is self-preservative, but the new forms are interwoven



FIG. 4.

with the old in a way that indicates how important results may follow from the appearance of the intruding stimulus.

Very interesting, finally, as involving useful variations of

the habits first present, under the influence of stimulation, is the twelfth case,¹ with which I will close the present exhibits. In this case the stimulus causes entirely new forms to appear in the third column. These forms and a certain combination of them with earlier forms in the fourth column show a decided increase of inventiveness.

So far for the cases themselves, in so far as I can here show them. Meanwhile, a few words as to the significance of these processes. You have in these instances a certain group of more or less subconscious habits—habits rather characteristic of an individual subject, called forth somewhat at random from a mass of motor-habits by the conditions under which the experiments begin. The subject is told to vary these habits; he does so independently. His variation usually involves an elaboration of them, but ere long comes to its limits, or threatens to do so, in consequence of the fact that he is forbidden to draw an object. Then you introduce a new stimulus. This stimulus is the presence of a definite object and the giving of a suggestion, intended to stimulate in its very vagueness a sort of suggestion that social conditions frequently give, and a sort of suggestion, too, that in history appears, when developed in more rational and less abstract forms, as of great importance for the development of inventiveness. This suggestion is: Look at your object, and in the presence of that object preserve your independence and express yourself in an individual way in contrast to that object. Now, this suggestion with many of my subjects has worked to make them merely self-preservative. That is, they have repeated their own former deeds, and thus have accomplished the unlike. But in a certain number of cases they have shown at least at first that the stimulus presented shatters their already formed habits. They must do something different. What this something is, the dim suggestions derived from the object and the general sensitiveness produced by the shock of change, seem to determine in a rather chance way. The notable fact is that a

¹ This case is reproduced in Fig. 4. The cases from the fourth to the eleventh, inclusive, could not here be reproduced; and I regret to have to leave my report of those cases to be accepted, or doubted, by the reader, without any direct control. My purpose in giving the mere reports of these cases here, apart from the plates, is simply to indicate the range of variation of the facts.

considerable alteration is very frequently wrought in attitude and matter by the coming of the intruding stimulus, and this alteration is due unquestionably to the fact that one is to maintain one's self against an intruder and is not permitted to imitate. In the cases more favorable to inventiveness, and these cases are not few amongst the cases studied by me, the result is that the subject combines his old habits with the new impression, in such wise as to become distinctly more inventive.

But what interests me most in the situation thus observable is the fact that three distinct kinds of results are produced by the intruding stimulus. (1) In some of the subjects the intruding stimulus produces *mere variation or tendency to vary*. In so far it is like many another contact with the environment in instances where one is a little puzzled or confused, or is in general put upon one's mettle, but with the result that one is not crushed by the intruding stimulus, but is rendered more active. In such cases, stimuli of the sort that we are considering show themselves as simply tending to increase the variability of the habits at any moment at our control. They tend so far to make us fuller of resources, less creatures of routine, but also in so far the victims of chance. But this is not the only effect that appears in the cases now under discussion. For (2): The call to make the unlike resembles many other social stimuli in making some subjects *more critical, more cautious*, and in so far *less disposed* to make new and free variations of their former movements. In extreme cases the result may be to paralyze inventiveness altogether. But now are these two tendencies wholly opposed? This new tendency is a tendency to check the freedom that appears in our second column's work in the four column cases. But it checks this freedom by making the subject more self-critical, more selective of his movements. Well, two such tendencies may well coexist; and in the case where normal individualism is encouraged by social stimuli whose general type my experiments have attempted to imitate in miniature—by stimuli, namely, that lead a man to maintain himself against social intrusion, to contrast himself with the environment, to hold his own against the critics—these tendencies do coexist. In some of my subjects they have done so. In

many of the experiments self-preservation and variation have gone altogether. In several cases the check which the intruders introduced soon gives place to a richer variety. But now (3) : In many cases the effect of the shock of difference is to lead to a combination of the old style with something new, and a blending of old and new elements which may involve a true invention.

In short, I submit that these experiments, not by proving any law, but by giving us symptoms of comparatively simple facts that are subject to analysis, do not indeed prove, but do in miniature illustrate a thesis whose proof I should leave to the whole history of invention. This thesis is that a particular type of social stimulation, a type prevalent in communities where individualism is encouraged, is productive of a threefold result. This type, namely (1), makes the habits of the individual more variable, by presenting to him manifold definite objects to think about in the acts of other individuals. In such individualistic communities, the individual, being encouraged to think about the acts of his fellows, and to criticise them, is led to be watchfully observant of a great many details of actions, just as my subjects were observant of the details of the objects shown them. Where such observation leads merely to imitation, the habits of the observing individual vary in only such ways as lead him to conformity with the social order ; and in a great part of everybody's life this is the result of social observation. But in the individualistic communities the social stimuli involve the suggestion that one must hold his own, must not do quite as the rest of the people do, must use whatever motive of variation the feeling of time encourages. Criticism, argument, controversy, effort at eccentricity in conduct, on occasion even gossip and scandal, or the love within normal limit of the extraordinary generally, will involve such dispositions. That is, dispositions to produce the unlike, are so far causes of social variation in such organisms as are adapted to independent variability in any sense at all. But so far this variability gives the mere material out of which inventions may come. (2) Notable in the social situations of which our experiments are a miniature illustration, is the presence of tendencies towards a very rigid selective or self-critical

inhibition of variations that otherwise exist. So far the same conditions which favor variations may also favor rigid selection. (3) The combination of these two tendencies may produce the most remarkable results. One may observe that in normal social relations the individual, brought in contact with foreign models, against which he must maintain himself, is required to vary and, at the same time, to bring his varying habits into some sort of combination with the already highly organized habits of his social environment.

It is easy to illustrate all these three tendencies from life. An individualistic community is very keenly selective of the individuals whom it will permit to continue their variations. It encourages variation, but also destroys by stern selection the kinds of variation that do not contribute to the general organization of the current social interests. This is the case even in a fairly successful debating club, where everybody is encouraged to break the routine of debate, while nobody is tolerated who is not a successful speaker. In consequence, the social situation of the sort prevalent in an individualistic community involves altogether three kinds of motive. First, by calling upon an individual to do the unlike under the definite stimulation of closely observed models, the individual habits are set to varying. Our experiments illustrate in a miniature instance how a variation that one could not produce alone is instantly produced by such a definite social stimulus. Of course, such variation is not always produced; but, in view of the rarity of true originality, a very few instances in such experiments as mine would be enough to illustrate very instructively what may happen in consequence of stimulating suggestions. In the second place, the very stimulation that puts one upon one's mettle, and calls out the unlike, renders one more keenly critical, more rigidly selective of one's own variations than one would be if left to one's self. And, finally, the fact that the social stimuli are already those which result from pretty highly organized social conditions assures the possibility of important combinations of the new and the old, such as occasionally appear even in our miniature experiments. And these may involve increased inventiveness.

But I said that I should for the most part only suggest prob-

lems. I point out, at any rate, where the problem lies. Our knowledge of the variability of habits, and of the causes that make the variation of habits, either in the individual or in society, valuable, is a knowledge still in its infancy. I have endeavored to show that there are conditions prevalent in individualistic ages and societies which involve an increased variability and an increased selectiveness in the habits of all concerned. Using a few illustrative and by no means final experiments, I have tried to indicate a way in which, perhaps, some more ingenious student of this subject may be able to organize an experimental investigation of the conditions that render our habits variable, and especially of those social conditions which, as I believe, the study of the history of human originality will show to be of the first importance for the comprehension of the psychology of invention.

PROCEEDINGS OF THE SIXTH ANNUAL MEETING
OF THE AMERICAN PSYCHOLOGICAL ASSO-
CIATION, ITHACA, NEW YORK, DE-
CEMBER, 1897.

REPORT OF THE SECRETARY AND TREASURER FOR 1897.

The sixth annual meeting of the American Psychological Association was held at Cornell University, Ithaca, New York, December 28, 29 and 30, 1897, the same time and place having been chosen by the American Society of Naturalists and the Affiliated Societies. There were thirty-eight members in attendance at the meeting and a considerable number of visitors at the various sessions.

In the distribution of papers at the sessions the lines between the different interests represented were somewhat more sharply drawn than formerly. Tuesday morning, the first day, was given up to papers of an experimental character; in the afternoon the Association was divided into sections, Section A meeting for the discussion of physical and mental tests, and Section B, with Professor Creighton in the chair, for the reading of psychological papers. On Wednesday morning, Professor Baldwin being prevented from reading his presidential address on 'Selective Thinking,' the address had been printed and distributed to the members, and with this as a basis there was a discussion on 'Invention,' opened by Professors Royce and Jastrow and Dr. Urban, and continued informally by various members of the Association and visitors from other societies, following which the regular business meeting of the Association was held. In the afternoon of Wednesday the Psychologists adjourned to meet with the Naturalists for their discussion on 'The Biological Problems of To-day,' Professor Cattell presenting the psychological aspect of the question. The final session on Thursday morning was given up to papers of a strictly philosophical character. The members of the Association were

present on Tuesday evening, at the reception given by President Schurman, at his residence to the visiting societies and at the annual dinner of the Affiliated Societies on Wednesday evening. President J. Mark Baldwin presided at the meetings of the Association.

At the regular business meeting and in the intervals of the program the following business was transacted: Election of officers for 1898: *President*, Professor Hugo Münsterberg, Harvard University; *Secretary and Treasurer*, Dr. Livingston Farrand, Columbia University; *Members of the Council*, Professor J. E. Creighton, Cornell University, in the place of Professor Royce, resigned, and Dr. A. Kirschmann, University of Toronto, and Professor E. B. Delabarre, Brown University. Elected to membership, Mr. A. H. Abbott, University of Toronto; Professor F. B. Brandt, Philadelphia High School; Professor William Caldwell, Northwestern University; President Charles DeGarmo, Swarthmore College; Professor W. G. Everett, Brown University; Mr. S. I. Franz, Columbia University; Professor William A. Hammond, Cornell University; Dr. John P. Hylan, University of Illinois; Dr. J. A. Leighton, Hobart College; Professor J. E. Russell, Teachers' College; Dr. Albert Schinz, Clark University; Professor Frank Thilly, University of Missouri; Professor E. B. Titchener, Cornell University; Dr. Wilbur M. Urban, Princeton University; Professor R. M. Wenley, University of Michigan.

The Council recommended the following amendment to the constitution, to be acted upon at the next annual meeting, viz.: That, hereafter, the Secretary be elected for a term of three years and be *ex officio* a member of the Council.

On recommendation by the Council, it was voted that a sum not exceeding one hundred dollars be appropriated for the use of the Committee on Physical and Mental Tests.

With regard to the nomination and election of new members, it was resolved: That nomination blanks be provided by the Secretary with spaces for the name, official position, and publications of the candidate and the names of two proposers, members of the Association; such blanks to be filled in and sent to the Secretary before the meeting and to be read before the

Association when the name of such candidate comes up for election.

With regard to the question of a summer meeting, the following resolution was proposed by Professor Cattell and carried: Resolved, that the American Psychological Association hold a meeting for the reading of papers at Boston, in 1898, at the time of meeting of the American Association for the Advancement of Science, the papers to be presented either before the Section of Anthropology of the American Association or to the Psychological Association alone as may appear more desirable to the Council.

It was decided that the next annual meeting should be held at Columbia University, New York, that place having been chosen by the affiliated societies upon invitation from the President of the University.

After a unanimous vote of thanks for the hospitality extended by Cornell University and the Local Committee of Arrangements, the meeting adjourned.

REPORT OF THE TREASURER FOR 1897.

Livingston Farrand in account with American Psychological Association.

DR.

To balance at last meeting.....	\$458 54
Dues of members.....	261 00
Sale of Proceedings.....	1 50

CR.

\$721 04

By expenditures for

Postage, telegrams, etc.....	\$14 10
Stationery	11 10
Printing and clerical work.....	26 74
	<hr/> \$51 94

Balance on hand	\$669 10
Estimated interest on deposits.....	25 00

Approximate assets.....	\$694 10
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Audited by the Council and found correct.

LIVINGSTON FARRAND,
Secretary and Treasurer.

ABSTRACTS OF PAPERS.

President's Address: *Selective Thinking*.¹ By J. MARK BALDWIN, Princeton University.

"By 'selective thinking' I understand *the determination of the stream of thought*, considered as having a trend or direction of movement, both in the individual's mental history and also in the development of mind and knowledge in the world." "We are led to the following rough but serviceable division of the topics involved—a division which my discussion will follow—namely: 1. The material of selective thinking (the supply of 'thought variations'); 2. The function of selection (how certain variations are singled out for survival); 3. The criteria of selection (what variations are singled out for survival); 4. Certain resulting interpretations."

The conclusions are as follows: "Selective thinking is the result of motor accommodation to the physical and social environment; this accommodation taking place in each case, as all motor accommodation does, from a platform of earlier 'systematic determination' or habit. In a sphere of the physical environment as such, the selection is from over-produced movements projected out from the platform of the habitual adaptations of the members brought into play; in the sphere of the social environment it consists in the accommodation of the attention, secured by the over-production of motor variations projected from the platform of the habitual attention complex. The presentations from which the selected motor variations issue are believed or called 'true,' while the organization which the motor complex gradually attains holds the data of knowledge in relations of theoretical and analytical 'validity.' In the case of physical selection the internal organization represented by systematic determination gradually serves to free the organism from direct dependence upon the control of the environment; in the intellectual life this is even more true, the development of the individual's judgment growing more and more independent of social control as progress is made in the 'systematic determination' which organized knowledge exhibits."

¹ Printed in full in THE PSYCHOLOGICAL REVIEW, January, 1898. The main points have been summarily stated in the writer's work on *Social and Ethical Interpretations in Mental Development*.

"We then need a theory of evolution which will account for the determination of race-progress in the lines thus marked out by the individual. This requirement is met, I believe, by the theory of Organic Selection, recently proposed by the present writer, considered as supplementary to Natural Selection in the way of securing lines of determinate evolution. According to this view, those individuals which successfully accommodate to the environment live and keep alive through heredity the congenital variations which they exhibit. To these are added further congenital variations which are again selected. Thus variations are secured in definite lines in a series of generations—lines which reproduce the determination first secured in the individual under the control of the environment. On this view, there would be a constant selection of individuals by natural selection, from a platform of organic selection which is analogous to the platform of 'systematic determination' in the individual. Race evolution would thus, on the whole, conform to the exigencies of experience, and would seem to be directly controlled by the environment, while due, in truth, to a series of variations accumulated by Organic and Natural Selection."

Fluctuation of Attention. By J. P. HYLAN, University of Illinois.

The aim of this study is to help solve the psychological problem involved in the inconstancy of our feelings towards persons and things, and elucidate the meaning and value which change and diversion have for mental activity.

In ergograph experiments, in which a single muscle is worked to exhaustion, there is evidently a fairly constant supply of energy, and the time needed for exhaustion depends upon the ratio between the rate of this supply and the rate of its use. By selecting a mental activity in which variety is restricted, as in adding columns of figures, the variations of the rate in this activity seem to indicate the same laws to hold here, although the variety in the different possible methods of adding introduced by the subject vitiates to a degree the evidence that the same laws hold strictly.

Other experiments were performed to test this further, and

with positive results. It also seemed evident from another series of experiments that the complexity and æsthetic value of the object of attention—or the increased variety of the mental activity—tends to obscure the laws which determine the direction and intensity of the activity. The theory which these experiments evidently support is that each object of attention innervates certain nervous elements in the cortex, distinct to a considerable degree, from those innervated by other objects, and that the comparative exhaustion of one set of elements causes another set to function and the direction of the attention to change, or fluctuate, in accordance with this change of function.

The Visual Perception of Depth. By CHARLES H. JUDD, Wesleyan University.

This paper aims to show that there is no direct perception of depth by means of the sensations of a single retina unaided by sensations of movement or by binocular factors.

If half of one of the figures seen in a stereoscope is covered, the depth is no longer seen on that side, and the whole horizontal extension of the image is interpreted as horizontal extension of the object. This is possible since horizontal extension of the object and depth are both represented in the image by horizontal extension.

A system of objects whose sizes are unknown but whose depths are different will appear to be in a single plane when seen monocularly.

If two threads be arranged so as to cross each other at an acute angle and to lie at different depths, and if an observer look down along the length of these threads, he will note that one of the double images of the upper thread extends through the third dimension away from him into the plane of the lower thread. The converse is true of one of the double images of the lower thread. The explanation is that the crossing points for the two eyes are different. They do not fuse, and when both eyes are open we have a binocular field with two monocular points. Binocularly there is a discrimination of depth; monocularly there is none. The illusion is the result of the necessity of satisfying both the monocular and the binocular conditions.

Third, this principle of non-discrimination of differences in depth in monocular vision explains the fact that objects seen beyond the point of fixation with a single eye, or crossed double images seen with two eyes, appear to decrease in size with increasing accommodation or convergence. The spreading apart of double images is due to an interpretation of horizontal extension in the image as horizontal extension of the object, not in accordance with its true significance as representing both horizontal extension and depth.

a. The Time of Perception as a Measure of Difference in the Intensity of Sensation.

b. A Method for Studying Muscular Fatigue in its Relations to Mental Conditions. By J. McKEEN CATTELL, Columbia University.

(a) Experiments were described showing that the time of discrimination increases as the difference in the intensity of two sensations is decreased. The time of discrimination can thus be used as a measure of the amount of difference in the intensity of sensation, which adds a new method to the 'error methods' of psycho-physics. The method can further be used for large differences in sensation, where the results of the psycho-physics methods are questionable. The stimulus used was light reflected from gray surfaces, and it was explained how a series of grays from a normal white to a complete black had been prepared and photometrically determined. The gray surfaces, 211 in number, had also been used to study the error of perception made in arranging them in the order of brightness.

(b) The importance for psychology of problems connected with movement and with the relation of movement to sensation and emotion was emphasized, and it was argued that questions of fatigue, especially the relations of muscular to mental fatigue, were of theoretical interest and had certain practical applications in schools, and indeed in the conduct of all mental and physical work. The subject had been put on an experimental basis by the ingenious invention and work of Mosso, and the research now reported on by the speaker, which was being carried on in conjunction with Mr. Franz, was in continuation of this. An

instrument was exhibited in which a spring dynamometer was substituted for the lifted weight used by Mosso, and it was claimed that this method had many theoretical and practical advantages over the use of a lifted weight. Curves were exhibited showing the course of fatigue under varying circumstances. The results did not altogether confirm those of Mosso, and the work is being extended in new directions.

Recent Investigations at the Yale Psychological Laboratory.

By E. W. SCRIPTURE, Yale University.

A number of investigations finished or in progress were briefly summarized; the complete reports will be published in the 'Studies from the Yale Psychological Laboratory.'

1. Reaction-time in Abnormal Conditions of the Nervous System. Simple reaction to a suddenly exposed red disc and complex reaction to one of two discs (thus involving discrimination and choice) were measured on patients at the Yale Dispensary. The results are given in the following table:

	<i>S</i>	<i>U</i>	<i>V</i>	<i>C</i>	<i>U'</i>	<i>V'</i>	<i>B</i>	<i>k</i>
Local neuritis	360	42	67	570	63	135	210	11
Multiple neuritis	418	35	4	848	63	54	430	3
Locomotor ataxia	387	8	7	786	23	42	399	4
Alcoholism	163	13	7	440	26	48	277	17
Hysteria	192	53	51	671	100	96	479	12
Normal	179	29	31	349	58	58	170	19

S, simple reaction-time.

C, complex reaction-time.

U, *U'*, average of the individual mean variations.

V, *V'*, average departures of the in-

dividuals from the typical averages *S* and *C*.

B, difference between *C* and *S*.

k, number of individuals; 40 records on each.

2. Effect of Sending the Electric Current Through the Brain. Measurements on simple and complex reaction-time with and without the passage of a constant current through the temples (unknown to the subject). Result: General shortening of time.

3. Influence of Regularly Repeated Stimuli. The stimulus was the flash of a Geissler tube at intervals of two seconds. The moment of pressing the reacting key and the moment of releasing it were recorded. The flash was observed with both

eyes in a dark room (involving convergence, accommodation and attention), with one eye (involving less accommodation), in a lighted room with eyes directed to the tube (involving no special act of accommodation or convergence, but chiefly an act of attention). The fatigue, which somewhat resembled that of being hypnotized by repeated stimuli, was judged by the lengthening of the time of reaction and the time of holding down the key. Result: fatigue increases with the complexity of the adjustment, being greatest in the first form of experiment; the tendency to fall into a dazed condition depends mainly on a fatigue of the attention.

4. Effect of the Energy of the Reacting Movement. The greater the energy of the movement used in reacting, the shorter the time.

5. Weber's Law in Illusions. The least perceptible difference in weight was found for three cylinders of the same weight but of different sizes; the fraction varied inversely as the size, but was less than the regular amount for weights when the size is unknown.

6. New Dynamometer and the Scale of Effort. The thumb and index finger are pressed on small knobs borne by two steel rods; the amount of movement is small, while the scale can be made very accurate. To transform the psychophysical measurements into purely psychological ones, it is proposed to have the subject give his own scales of pressure in the relations of 1, 2, 3, 4, etc., and to reduce all readings to the scale.

7. Among the investigations in progress mention was made of one on the localization of sounds in space, another on the laws of practice and habit, another on the psychology of reading, and another on the relation of emphasis to length of syllable in English poetry.

8. A lamp battery was exhibited.

Class Experiments. By A. KIRSCHMANN, University of Toronto. (Read by title.)

Time Measurements of Visual After-images. By SHEPHERD IVORY FRANZ, Columbia University. (Read by title.)

Recent Discussion of Color Theory. By MRS. CHRISTINE LADD FRANKLIN. (Read by title.)

Color Saturation. By ALBERT H. ABBOTT, University of Toronto.

The paper dealt with the general problem of saturation, and reported results reached by experimenting with discs constructed so as to show the same intensity (brightness) over the whole disc, the same color-tone, and a gradual transition from the full color-tone to gray; thus isolating saturation changes. (The method of construction of such discs was described in an article by Kirschmann, *American Journal of Psychology*, Vol. VII., No. 3.)

In leading up to the problem a general reference to the color cone as a representation of the three-fold variability of light and color sensations, as this has been changed by Kirschmann, was made, and a colored diagram of the cone was shown. In this connection the possibility of illustrating various phenomena by means of the cone was noted and an attempt made to define the highest saturation of colors. This was stated to be, "those color qualities or that saturation of those qualities which cannot be changed in the way of being made brighter or darker, without lessening the number of transitions possible between them and gray or colorless light of the same brightness as those qualities."

The general result of the investigation, which was carried on according to the method of average errors, were given, after some of the difficulties of experimentation had been noted.

These difficulties may be summarized as follows:

- (1) Inconstancy of light intensity.
- (2) Shadows from parts of the apparatus.
- (3) Unequal illumination of the two sides of the disc.
- (4) Errors due to position of arm and head in making adjustments to find the saturation required.
- (5) Difficulties regarding the space threshold and the threshold proper of saturation.

The general summary of results is:

- (1) For low saturations the errors are very great, being from 10 % to 15 %.

(2) For high saturations the errors are very small, being from 1 % to 2 %.

(3) Saturation cannot be considered as an intensity, pure and simple, since Weber's law does not hold, as above.

(4) For intermediate saturations Weber's law is approximated.

Experiment in the Psychology of Perception. By BROTHER CHRYSOSTOM, Manhattan College. (Read by title.)

The Place of Experimental Psychology in the Undergraduate Course. By F. C. FRENCH, Vassar College.

The few remarks which I have to make on the negative side of this question are intended to elicit discussion, and in no wise to disparage the value of experimental psychology—the place for which I believe to be in the university or graduate school—and not in the college or undergraduate course. As far as the general student is concerned we believe that such time as can be devoted during the college course to scientific work, whether for the purpose of scientific training, or that general knowledge of nature such as should be demanded of every college graduate, would better be spent in the study of the sciences already well developed. The time at his disposal can be better employed on such subjects as physics, chemistry, biology, astronomy, etc., whose fundamental principles are already ascertained, and whose general pedagogical procedure is already well established. Such time as he has for the study of mental phenomena will hardly exceed what is necessary for a good course in general psychology. Now, if it is granted that there is no place for experimental psychology in the course of a general student, is it not demanded for those few students who may be intending to specialize in psychology later? It seems to me—not. Experimental psychology is a science of the crossroads—it occupies the ground where physics, physiology and general psychology meet. The best foundation therefore which a student can lay in his college days for original work in this subject later, is to acquire as broad and thorough a knowledge of and training in physics, physiology and general psychology as possible. Thus

we conclude that, from the point of view of both the general and the special student, there is no call for experimental psychology in the college, but that its proper place is in the graduate school, and that the true interests of the science will be furthered by recognizing this.

Immediacy and the Concept of Sensation. By EDGAR A. SINGER, JR., University of Pennsylvania.

Our ordinary insistence upon the *constructive* aspect of scientific thought furnished the starting point for the inquiries presented. Construction, as standing for the body of inferences by which science is conceived to erect, on the basis of given facts, a complex system or world, seems to require as its beginning some immediate datum. It is usually assumed that we have the right to ask: what are these 'mere facts,' given in experience, from which construction is to proceed? Assuming that there is meaning in this search for our data, the task of finding them has proved difficult, or even impossible of accomplishment. Comparing this 'destructive' with the fore-mentioned 'constructive' function of science, we seem to find ourselves involved in a double paradox. To state the problem for constructive thinking would seem to require certainty respecting that which is *given* as a basis for our inference. Yet it is claimed (1) that we can, or even must, occupy the attitude of *seekers* for that which *ex hyp.* is *given* to begin with, and (2) that, although our search must have a real object, it is difficult, or even impossible, to discover it.

Without questioning the right of search, the paper proceeded to show that the ordinary method of search must be fruitless. This method was said to involve an effort to abstract from any term of our thought, all elements that could be shown to depend on inference, and to regard what then remained as merely 'given.' Sensation has very generally been taken to be this given element. But the following objections were urged: (1) The ordinary meaning of sensation is incompatible with the ideal of immediacy. (2) An attempt to narrow the meaning of the term—*e. g.*, to that of 'pure quality' (Helmholtz)—does not succeed in removing constructive elements so long as any mean-

ing is retained. (3) The outcome of the method must be to regard sensation as a limit, unattainable save as an abstraction. But here, besides having emptied a useful term of its content, we have only emphasized the paradox of regarding the immediately given element of experience as a quite unattainable product of abstraction. Nor can we escape these difficulties by substituting for sensation any other term.

It was pointed out, however, that the real paradox did not lie in the failure to find the immediate, but in the undertaking to search for it. It was suggested that our reasons for such an undertaking rested on a misconception of the problem of scientific thought. Constructive and destructive this is, in its different aspects; but the entire nature of such thought is better said to be reconstructive. In the only sense in which reflective thinking can be said to have a starting point which is not an object of search, its starting point is the whole common-sense world of the naïve consciousness. As a result of reconstruction this world appears a construct, but the assumed data on which the construction proceeds are themselves products of reflection. The condition of scientific progress is that no such data should be ultimate. Those that are taken as a basis of construction are assumed from a point of view, frequently an historical accident. The assumption that an absolute datum must exist has its origin in a confusion between the beginning of reflection (on historical fact) and a datum for construction (a result of reflective analysis). It has its logical end in 'mere abstractions.'

The Intellectual Content in Dream Consciousness. By ROBERT MACDOUGALL, Western Reserve University.

A strong but utterly vague impression of the number and variety of dream images is a frequent accompaniment of our reminiscent consciousness, and we are in the habit of attributing to the dream which has escaped us a high ideational value in justification of the very real emotional overtone which has remained. This we do on the analogy of our waking consciousness. All our acute and significant emotional experiences rest upon an intellectual content. Our human grief and joy root in action and the drama of life; they are significant and reason-

able, and this canon we apply to the interpretation of our dreams. It is the truth of this interpretation which I question, especially as regards those dreams characterized by great time content. It is doubtful whether these dreams ever embrace an ideational content which in the wealth and variety of its happenings would require years *to act out*. The details which we recall are always meagre; we get a few fragmentary ideas, a vague fringe-feeling that vastly more was present than we can recall, and a vivid impression of a great time-occupying extent. I believe that this *impression* is the essence of the dream. The physiological facts are directly hostile to the hypothesis of a rich ideational content. The conditions of rapid and intense brain action are lacking, and if the intensity of thought action is proportionate to the activity of the brain centres it must be more sluggish than in waking life; yet the common belief in the swiftness of the dream procession demands a functioning incomparably more rapid than this. We should seek the type of such dreams in that form of nightmare in which the subject hangs for countless ages in horrid and unbearable suspense. The monster *seems* to pursue us century after century, not because of the number of ideas which succeed one another in the experience, but because of the intensity of the emotional excitement; it seems an age because the actual moment was one of unendurable agony. This, it seems to me, is the essence of all such dreaming. The dream consists of a succession of intense states of feeling supported by a minimum of ideational content. Its reality is not an *idea-multiplicity*, but a *feeling-intensity*, and the notion of an intellectual background upon which this feeling rests is an illusion. *The feeling is primary: the ideational content is the inferred thing.*

Professor Titchener's Treatment of the Self. By WILLIAM CALDWELL, Northwestern University.

This paper discusses what its author begs to call the 'limitations' of Professor Titchener's view of the self, as expressed in his 'Outline of Psychology.' (For the positive portions of that book, its actual work in Experimental Psychology, the writer has no feeling but that of indebtedness and admiration.) In

the 'Outline' there are two methods of treating the 'self': (1) that of positive presentation as a 'sum total of conscious processes, etc.' (pp. 287-292); and (2) that of the discussions of hypotheses, denying that there is psychological evidence of a 'mind behind' processes, or of 'mental activity,' or of 'mental continuity and coherence.' In regard to (1), the paper maintains that T.'s positive treatment is good enough as far as it goes ('as psycho-physics') but that it does not go far enough. In regard to (2), is not the fact that T. finds himself compelled to discuss theories about the ultimate nature of the self, itself a fact of psychology? The admission (made by T.) that psychology alone is inadequate to setting forth the nature of the self, ought to have prevented him from saying that the self is only tendencies and dispositions.

Some reasons why T.'s account of the self is inadequate are: (1) He professes to analyze effort (p. 123, etc.). He really analyzes only desire. (2) He confesses that his *apparatus* for the examination of effort and attention is one that could show in them nothing but sensation and affection. Let him change his *apparatus*. (3) It is the 'activity-experience' that gives rise to the 'activity-inference'—not *vice versa*. (4) The 'time-experience' in volition introduces the element of an ideal self that *affirms* itself as *active*. Omitting the higher forms of attention, T. omits this. (5) He leaves out of his consideration many things that other psychologists find in the self. (6) His 'biological self' is as lacking in 'unity' as his 'psychological' self. (7) He keeps to mere 'presentationism.' (8) His treatment of feeling is inadequate. (9) And of the 'unconscious.' (10) He does not allow for the fact (admitted by him) that in the origination, *etc.*, of mental states we are *ourselves* concerned. (11) Psychical manifestations are nothing apart from a manifesting self. (To be printed in THE PSYCHOLOGICAL REVIEW.)

Aristotle's Doctrine of Ψυχή as Biological Principle. By WM. A. HAMMOND, Cornell University.

Aristotle's long-neglected treatises, the *Metaphysics* and the *Psychology*, are beginning to receive a large share of attention

from professional students of the history of speculation and scientific observation. This is due partly to the present widespread interest in psychology and partly to the universal employment of the historical or, as it has sometimes been called, the biological method in investigation. In the development of thought there has hardly been a phase so significant for the subsequent life of speculative or empirical science as Aristotelianism. This is due in part to Aristotle's wide employment of observation and induction; in part to the fact that he has interpreted to us the philosophical import of Greek ideas in art, politics, morals and physics. Aristotle's conception of the soul and his treatment in general of psychological problems are intimately connected with his work in biology and natural history. His psychology is a part of his physics (*φύσις*), and to him is one of the natural sciences. All the treatises of Aristotle which deal with the soul or mind would best be called treatises on the life-principle. Soul and life to him are identical terms. The distinguishing mark of living organisms is the possession of *Ψυχή* or soul. This soul consists essentially in the power of self-movement or the native capacity of a thing to work changes within itself. Different types of life effect different types of change, the primary form of which is the change of growth and decay effected by vegetal life. Aristotle, as is well known (and this is characteristic of the Greek philosophy in general), conceived of the processes of nature as moving in an ascending scale from the simplest to the most complex forms of animate existence.

Soul is the unitary principle in which the manifestations of vegetal life, sense-perception, and thought are embraced. In every organism the soul is a unit. It is, to use Aristotle's words, 'the entelechy or complete realization of a natural body endowed with the potentiality of life.' The soul itself is not corporeal, although it does not exist apart from the body. Body and *Ψυχή* are distinct things only logically, not existentially. Of the three main anatomical divisions in the higher animal organisms, viz.: head, thorax and abdomen, Aristotle regards the thorax as the seat of the life-principle, *i.e.*, the physiological center is also the life or psychical center, which comports with his definition of soul. Its special organ is the heart.

Epistemology and Theories in Physical Science—A Fatal Parallelism. By A. H. LLOYD, University of Michigan.

Epistemology and physical science are still separated by a fundamental dualism of mind and matter, but each one of these sciences, quite within its own sphere, has reproduced the separating dualism. Thus, epistemology has an abstraction for matter in its sensation as element or stimulus or continuum or only formal relation, and physical science has an abstraction for mind in the conserved matter of chemistry or the matter as medium of physics or the infinity as quantity of mathematics.

The mathematico-physical undulatory theory, moreover, not only is dualistic, but also completely reflects the current Kantian epistemology, the vibration as now having a fixed amplitude and duration and now indefinitely transmitted or perpetuated, being a perfect incarnation of Kant's antinomy of space and time as now finite or finitely divisible and now infinite or infinitely divisible. Thus, according to the undulatory theory, a motionless non-temporal, non-spatial medium has qualities that are generated by motion—necessarily vibratory—in a formal space and time; and this is sheer Kantianism.

And the parallelism between epistemology and physical science is fatal both to the separating dualism and to the reproducing theories. The persisting dualism, which is in general that of the simple and the composite, is in itself evidence that the real, the substantial, can be neither the simple, whether as immaterial mind or as abstract matter, nor the composite, whether as sensations or as atoms, but must be the organic as the true unity of the simple and the composite. In the organic, mind and matter are one; and one also are conserved matter and atoms, medium and motion, infinity and quantity, as well as conception and sensation.

But biology, science of the organic, is already appealing to physical science for light upon the central biological problems, and this appeal is very significant, not to say grimly humorous, since epistemology at the present time is appealing to biology, and since biology in its 'vital unit' has been thinking of the organic under a purely physical guise. The 'vital unit,' however, has not yet been found; and the time cannot be very far

off when physical science will not only receive, but also, through a consciousness of its own error, wholly forgive the appealing prodigal.

(This paper will be published in full in the *Philosophical Review*.)

Romanes and Mill. By JOHN GRIER HIBBEN, Princeton University.

In the experiences of Romanes and Mill there was an evident transition from a negative to a positive position in reference to theistic belief, in attentative manner as regards Mill, and in a frank acknowledgment on Romanes' part of a radical change of opinion. This is seen in Mill's essay on *Theism*, as contrasted with his earlier essays on *Nature*, and on the *Utility of Religion*. In Romanes' case, we find his *Thoughts on Religion* repudiating the conclusions of his early work on *A Candid Examination of Theism*. The psychological interest in these changes of belief lies in the fact that both Mill and Romanes confess to a neglect of the cultivation of the feelings in their early education, and in their subsequent habits of thought and work, but later in life each allowed the sentiments and emotions fuller play and scope, with a consequent deepening and expansion of the religious consciousness.

Inasmuch, therefore, as there seems to be a place for the extra-logical within the consciousness of severely logical and scientific minds such as Romanes' and Mill's, and yet no conscious incongruity be experienced, it is possible that the extra-rational is not necessarily irrational, but may become incorporated in the body of rational doctrine.

Contributions of Psychology to Morality and Religion. By JAMES GIBSON HUME, University of Toronto.

An appreciative statement of the chief results of recent advances in psychology and an endeavor to justify a still wider application of thorough psychological analysis, experiment and theoretical reconstruction.

Former objections to experimental psychology from introspective psychologists and natural scientists were due to a misunderstanding and to an abstract dualistic theory.

The psychologist is not merely entitled to a scientific field beside others, he should claim all psychical facts directly accessible to his inquiries, and should contribute toward the reconstruction of other sciences.

He should assist the natural scientist in guarding him against the misconceptions of materialism. A psychology that takes its stand upon the actual, concrete, active self is the most positive refutation of the abstractions of materialism and pantheism. This self-revealing active self enables us to meet the old difficulties of ethics with new insight. Theory must not be separated from practice. Neither should morality be separated from religion nor religion from morality. Religion includes and transforms morality. The Divine Being is personal and in social coöperation with the struggling finite moral self.

At the meeting of Section A for the discussion of Physical and Mental Tests, Professor Cattell as chairman presented the report of the Committee of the Association; Professor Jastrow spoke on Popular Tests of Mental Capacity, and the discussion was carried on by Professor Baldwin and others, being reported at length in the present number of the REVIEW.

The papers by Professors Royce and Jastrow, with which the discussion on Invention was opened, are published in full in THE PSYCHOLOGICAL REVIEW; an abstract of Dr. Urban's paper follows. The discussion was continued informally by Professor Baldwin, Professor W. H. Brewer, Dr. W J McGee, Dr. Franz Boas and others.

Concerning the Limits of the Application of the Utility-Selection Hypothesis to Mental Phenomena. By WILBUR M. URBAN, Princeton University.

This paper aimed to show, by a critical and historical study of the concepts of 'Accommodation' and 'Utility-Selection,' that the logic and actual practice of thinking of mental phenomena in these terms has led to a more and more psychological interpretation of them. Instead of psychological 'accommodation' being but a form of the general biological principle, having as its postulate the mere persistence of the biological organism in

its environment, therefore biological well and ill-being, reducible in the last analysis to physical causation, a critical study has led more and more to the postulate of an original psychical energy which accommodates to psychological elements. The theories of Baldwin, Stanley and Simmel were cited as examples. The paper demands, as a necessary consequence of this movement, that the concept of '*utility*' should be freed from its biological limitations, and given a purely psychological interpretation, which upon analysis of consciousness is found to be the hedonistic principle of the summation of pleasures.

The tendency to make psychological terms—pleasure and pain—equivalent to biological well and ill-being is met by the well-known fact that the 'criterion' of worth, upon which voluntary selection takes place, is applied to pleasures independently of their biological significance. Especially is this true of the centrally-aroused pleasures, which in the selective functions of the reproductive processes play the chief rôle.¹ A psychological theory of selection must presuppose the psychical elements—such elements as analysis shows are not further reducible. From a purely psychological standpoint the problem of how these elements were originally selected for consciousness—why these and not others, is not soluble. An attempt at its solution involves monistic philosophical considerations in which the guiding concept is either one of biological utility—or some metaphysical logical principle—in either case unpsychological.

INFORMAL REPORTS.

Informal Report on Work in the Princeton Laboratory. J. MARK BALDWIN.

Three researches awaiting publication were reported upon. I. *An Investigation of Temperature Sensation*, by Mr. J. F. Crawford, of which a preliminary statement of results has now been made in THE PSYCHOLOGICAL REVIEW for January, 1898. It reports (1) a new method of procedure—that of using transparent 'transfer-frames,' whereby exact repetitions of each ex-

¹The writer called attention to the article in the PSYCHOLOGICAL REVIEW, for July 1897, 'The Psychology of Sufficient Reason,' where the standpoint is further developed.

ploration of the skin are secured, and (2) the result that the temperature stimulation does not take effect in spots, either for hot or cold, but in *areas* which are in general but not strictly exclusive for heat and cold.

II. *A Research on 'Type Variations in Reaction Times,'* by J. Mark Baldwin, which takes up two problems: (1) To ascertain how far the indications of mental type secured by differences in simple reaction time, as between 'sensory' and 'muscular' reaction, agree with the results of introspective determination of mental type (independently carried out on the same subjects). (2) To determine whether the differences between 'sensory' and 'muscular' reactions for the hand, to various stimulations, are the same in kind as the corresponding differences for speech in the same subjects. Many series of experiments were made on each of four subjects with the following general results. In each of the subjects *both the correspondences suggested above were found to hold*; the most striking case being that of Mr. J. F. Crawford, whose simple sensory reaction to sound and light is very much shorter than his muscular reaction, *for both hand and mouth reactions*,¹ and whose mental type, as independently determined by various introspective tests, *is unmistakably auditory*. In two of the subjects the agreement between hand and mouth reaction is negative, namely, there is no difference between sensory and muscular reactions for either function; and in these persons the same condition is reflected in their great difficulty in securing clear introspective indications of type. In the fourth case the subject finds himself visual in his type, and his reactions show sensory times slightly shorter than the muscular in both hand and mouth functions. It is remarkable that in these four subjects—three never having been tested before and the fourth only slightly—*there is no instance of muscular reaction shorter than sensory for either hand or mouth reaction to either sound or light*. The author considers the results as supplying important evidence of the truth of his type-theory of reaction. Full details of the investigation are to be published in an early issue of THE PSYCHOLOGICAL REVIEW (as also those of the topic next reported).

¹ A cut showing a new form of mouth-key was shown; it is figured in the *Intermédiaire des Biologistes*, March 5, 1898.

III. *An Optical Illusion*, by J. Mark Baldwin. This report gave the results of further experiments of the 'illusion' reported by the author in THE PSYCHOLOGICAL REVIEW, May, 1895, pp. 244 ff., *i. e.*, that the point fixed upon as the mid-point between two square areas is located too far toward the smaller area, the amount of the displacement increasing with the ratio of the larger to the smaller square side. The following new results are reported: 1. The earlier results are confirmed on additional observers. 2. The illusion holds, though much lessened in amount and with some exceptions, for the determination of the mid-point between two parallel lines of different lengths. 3. The illusion requires a figure of certain—not exactly determined—maximum size, *i. e.*, the illusion does not appear when the eyes of the observer are too near, but appears when he retires backward from the figure.

The author also reports having established the reverse illusion, *i. e.*, the misjudgment of the mid-point when actually marked, in the figure just described. It is held to lie *too far toward the larger square*. This result is completely established by returns in a great many answers to a request printed (in *Science*, November 27, 1896) with an accompanying figure—principally returns from students collected by teachers of psychology and science.

The author intends to investigate the effect upon the illusion of variations in the distance between the areas, and also to test various explanations of it. He has already found that the element of 'perspective' has probably little influence on it; also that the principle of 'equilibrium' does not account for it, seeing that (a) the illusion is contrary in its direction to that which this principle would produce, and that (b) it holds under conditions which exclude the operation of this principle. The fact of this reverse illusion establishes the point made in the original paper, that the experimental conditions—involving the following of a light-bead along the line—were, under the control methods employed, entirely adequate, and so meets the criticisms of Binet and Witasek. Certain æsthetic applications of the illusion will be indicated in the detailed paper.

Post-hypnotic Suggestion from a Dream. By ROBERT MAC-DOUGALL, Western Reserve University.

On the morning of Friday, March 19, 1897, I awoke suddenly and lay for some time unconscious of having dreamed. Four or five seconds after awaking I suddenly puffed my cheeks and blew out vigorously, as if trying to extinguish a light. The action was wholly unexpected and surprised me much, but at once it recalled a dream in the midst of which I had awaked. Just previous to the moment of waking I had sifted some white powder upon the surface of a glassful of water. The greater part of the substance had spread over the water in a fine powder, but some pieces remained in cohesion in the form of little pellets; and at the moment when my dream was broken in upon by the current of waking events I was about to blow the pellets about the glass to pulverize them.

There are here all the marks of a post-hypnotic suggestion. The break in consciousness was abrupt and involved a complete transformation in its content. The motor-idea remained dormant for a time; its acting out was thrust in among a set of unconnected mental conditions, and it was wholly unexpected and at the moment inexplicable. There was, on the other hand, no attempt to fit the act into the mental framework of the moment, such as often characterizes the post-hypnotic suggestion.

The incident is related in the hope that it may bring out like instances in the experience of others.

Professor Jastrow exhibited a sorting apparatus for the popular study of the time-relations of such processes as distinction and choice. The apparatus consists of a convenient group of compartments, into which cards bearing the objects to be distinguished (colors, letters, numbers, words, etc.) are to be distributed, each kind of object card being deposited in a designated compartment. The time of assorting the whole number of cards is recorded by the second hand of a watch, or by a stop-watch, and decided by the total number of cards to obtain the time of one 'sorting.' The method makes possible a great variety of tests of the rapidity of mental processes, and by

proper arrangements may be used to indicate the separate times of the several factors entering into a complex reaction. The apparatus will be fully described later.

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BALDWIN, PROFESSOR J. MARK, Princeton University, Princeton, N. J.
BIGHAM, DR. JOHN, DePauw University, Greencastle, Ind.
BLISS, DR. C. B., University of the City of New York, New York City.
BOAS, DR. FRANZ, Museum of Natural History, New York City.
BRANDT, PROFESSOR F. B., Philadelphia High School, Philadelphia, Pa.
BRYAN, PROFESSOR W. L., Indiana University, Bloomington, Indiana.
BUCHNER, DR. E. F., Yale University, New Haven, Conn.
BUCK, MR. A. F., Union College, Schenectady, N. Y.
BURNHAM, DR. W. H., Clark College, Worcester, Mass.
BUTLER, PROFESSOR N. M., Columbia University, New York City.
CALDWELL, PROFESSOR WILLIAM, Northwestern University, Evanston, Ill.

- CALKINS, MISS M. W., Wellesley College, Wellesley, Mass.
CATTELL, PROFESSOR J. McKEEN, Columbia University, New York City.
CHRYSOSTOM, BROTHER, Manhattan College, Grand Boulevard and 131st Street, New York City.
COWLES, DR. E., McLean Hospital, Somerville, Mass.
CRAWFORD, MR. J. F., 1060 N. Halstead Street, Chicago, Ill.
CREIGHTON, PROFESSOR J. E., Cornell University, Ithaca, N. Y.
DANA, PROFESSOR CHARLES L., Bellevue Hospital Medical School, New York City.
DE GARMO, PRESIDENT CHARLES, Swarthmore College, Swarthmore, Pa.
DELABARRE, PROFESSOR E. B., Brown University, Providence, R. I.
DEWEY, PROFESSOR JOHN, University of Chicago, Chicago, Ill.
DONALDSON, PROFESSOR H. H., University of Chicago, Chicago, Ill.
DUNCAN, PROFESSOR G. M., Yale University, New Haven, Conn.
EVERETT, PROFESSOR W. G., Brown University, Providence, R. I.
FARRAND, DR. LIVINGSTON, Columbia University, New York City.
FITE, PROFESSOR WARNER, Williams College, Williamstown, Mass.
FRANKLIN, MRS. CHRISTINE LADD, 1507 Park Ave., Baltimore, Md.
FRANZ, MR. SHEPHERD IVORY, Columbia University, New York City.
FRENCH, PROFESSOR F. C., Vassar College, Poughkeepsie, N. Y.
FULLERTON, PROFESSOR G. S., University of Pennsylvania, Philadelphia, Pa.
GARDINER, PROFESSOR H. N., Smith College, Northampton, Mass.
GILMAN, DR. B. I., Museum of Fine Arts, Boston, Mass.
GRIFFIN, PROFESSOR E. H., Johns Hopkins University, Baltimore, Md.
HALL, PRESIDENT G. STANLEY, Clark University, Worcester, Mass.
HAMMOND, PROFESSOR WILLIAM A., Cornell University, Ithaca, N. Y.
HIBBEN, PROFESSOR J. G., Princeton University, Princeton, N. J.
HINMAN, DR. ALICE HAMLIN, University of Nebraska, Lincoln, Neb.
HODGE, DR. C. W., Lafayette College, Easton, Pa.
HUME, PROFESSOR J. G., University College, Toronto, Canada.
HYLAN, DR. JOHN P., University of Illinois, Champaign, Ill.
HYSLOP, PROFESSOR J. H., Columbia University, New York City.
IRONS, DR. DAVID, University of Vermont, Burlington, Vt.
JAMES, PROFESSOR W., 95 Irving Street, Cambridge, Mass.
JASTROW, PROFESSOR JOSEPH, University of Wisconsin, Madison, Wis.

- JOHNSON, PROFESSOR R. B., Miami University, Oxford, O.
JUDD, DR. C. H., Wesleyan University, Middletown, Conn.
KIRKPATRICK, MR. E. A., Winona, Minn.
KIRSCHMANN, DR. A., University of Toronto, Toronto, Canada.
KROHN, PROFESSOR W. O., Hospital, Ill.
LADD, PROFESSOR G. T., Yale University, New Haven, Conn.
LEIGHTON, DR. J. A., Hobart College, Geneva, N. Y.
LLOYD, MR. A. H., University of Michigan, Ann Arbor, Mich.
LOUGH, MR. J. E., Harvard University, Cambridge, Mass.
MACDONALD, DR. ARTHUR, Bureau of Education, Washington, D. C.
MACDOUGALL, DR. ROBERT, Western Reserve University, Cleveland, Ohio.
MARSHALL, MR. HENRY RUTGERS, 874 Broadway, New York City.
MEAD, PROFESSOR GEORGE H., University of Chicago, Chicago, Ill.
MEZES, PROFESSOR SIDNEY E., University of Texas, Austin, Texas.
MILLER, DR. DICKINSON S., Bryn Mawr College, Bryn Mawr, Pa.
MILLS, PROFESSOR WESLEY, McGill University, Montreal, Canada.
MINOT, PROFESSOR C. S., Harvard Medical School, Boston, Mass.
MÜNSTERBERG, PROFESSOR HUGO, Harvard University, Cambridge, Mass.
NEWBOLD, PROFESSOR W. ROMAIN, University of Pennsylvania, Philadelphia, Pa.
NICHOLS, DR. HERBERT, 3 Berkeley St., Cambridge, Mass.
NOYES, DR. WM., Boston Insane Hospital, Pierce Farm, Mattapan, Mass.
ORMOND, PROFESSOR A. T., Princeton University, Princeton, N. J.
PACE, PROFESSOR E., Catholic University, Washington, D. C.
PALMER, PROFESSOR G. H., Harvard University, Cambridge, Mass.
PATRICK, PROFESSOR G. T. W., University of Iowa, Iowa City, Iowa.
PIERCE, MR. EDGAR, 3 Thompson Street, Ann Arbor, Mich.
ROYCE, PROFESSOR JOSIAH, Harvard University, Cambridge, Mass.
RUSSELL, PROFESSOR J. E., Teachers College, New York City.
SANFORD, PROFESSOR E. C., Clark University, Worcester, Mass.
SCHINZ, DR. ALBERT, Clark University, Worcester, Mass.
SCHURMAN, PRESIDENT J. G., Cornell University, Ithaca, N. Y.
SCRIPTURE, DR. E. W., Yale University, New Haven, Conn.
SETH, PROFESSOR JAMES, Cornell University, Ithaca, N. Y.
SHOREY, PROFESSOR PAUL, University of Chicago, Chicago, Ill.
SINGER, DR. E. A., University of Pennsylvania, Philadelphia, Pa.
SMITH, DR. W. G., Smith College, Northampton, Mass.
SNEATH, PROFESSOR E. HERSHEY, Yale University, New Haven, Conn.

STANLEY, PROFESSOR H. M., Lake Forest University, Lake Forest, Ill.
STARR, PROFESSOR M. ALLEN, 22 West 48th Street, New York City.

STRONG, PROFESSOR C. A., Columbia University, New York City.

TAWNEY, PROFESSOR G. A., Beloit College, Beloit, Wis.

THILLY, PROFESSOR FRANK, University of Missouri, Columbia, Mo.

TITCHENER, PROFESSOR E. B., Cornell University, Ithaca, N. Y.

URBAN, DR. W. M., Princeton University, Princeton, N. J.

WARREN, PROFESSOR H. C., Princeton University, Princeton, N. J.

WASHBURN, DR. MARGARET, Wells College, Aurora, N. Y.

WENLEY, PROFESSOR R. M., University of Michigan, Ann Arbor, Mich.

WILDE, DR. NORMAN, Columbia University, New York City.

WITMER, PROFESSOR LIGHTNER, University of Pennsylvania, Philadelphia, Pa.

WOLFE, PROFESSOR H. K., University of Nebraska, Lincoln, Neb.

Members will please notify the Secretary of any errors in names or addresses as given in the above list.

DISCUSSION AND REPORTS.

PHYSICAL AND MENTAL TESTS.¹

Professor Jastrow, of the University of Wisconsin, opened the discussion with a paper entitled 'Popular Tests of Mental Capacity.' He said that the determination of the extent, accuracy, alertness, efficiency and other measurable qualities of mental processes offers many interesting and perplexing problems to the practical psychologist. The primary motive in such experimentation is quite different from that which guides analytical research in the hands of the psychological expert. A very considerable and valuable portion of modern contributions to psychological principles and doctrine is the result of careful and ingenious analysis on the part of well-trained and scientifically self-observant experimentalists; and a main part of the equipment of a psychological laboratory may be wisely devoted to the purposes of such research. Supplementary to this is a line of investigation which aims to establish the normal capacity of simple and typical sensory, motor and intellectual endowments, as they occur in the average individual or in specially selected groups; such study, moreover, naturally includes as well the problems relating to the distribution of such powers, their development in child-growth, their relation to practical and daily pursuits, and their correlation with one another. As such investigation involves in most cases what is best described as a *test* of some specified capacity, and as such tests must be suited to the mental experience and endowment of the untrained and non-expert, the problem thus outlined may be appropriately referred to as the determination of the most suitable popular tests of mental capacity.

The problem falls into two divisions: (1) the selection of the capacities to be tested, and (2) the practical methods of testing them. The principles guiding the selection of tests will naturally vary with the special purposes for which various groups of tests may be undertaken. A usual and important purpose is that of collecting material for the study of general typical, characteristic endowment, much as the student of anthropometry desires first to establish standards of the princi-

¹ Abstracts of the discussion presented by members of the committee of the American Psychological Association at the Ithaca meeting.

pal dimensions and proportions of the human frame. The selection of typical measurements in physical anthropometry is a much simpler matter than in mental anthropometry. Weight, height, girth, muscular power and the like are obviously the chief respects in which our bodies differ. To a certain extent it may be possible to obtain a consensus of standard mental measurements; and this is one of the main purposes of the Committee appointed by this Association.

The tests, apart from a few personal and anthropometric data included to make possible a comparison between physical and mental endowment, fall naturally into (*a*) the senses, (*b*) the motor capacities, and (*c*) the more complex mental processes. Certain general desiderata may, perhaps, be suggested as applicable to each of these groups. It is well to have each test give information regarding a single or very limited group of powers; specific typical tests are better than general ones. It is better to select, even if in part arbitrarily, one form of a certain sense capacity and to test that sufficiently to yield a definite result, rather than to attempt to test superficially various forms of manifestation of the same function. It is important to arrange a test so that it is definitely clear just what the capacity tested is; and, if necessary, preliminary experiments of an analytical character should be performed to determine this point. It is desirable that the form of capacity chosen shall be related to the activities of daily life; but it is also desirable that a high degree of efficiency in any test shall not be dependent upon experience, which certain groups of persons have a decidedly greater opportunity of acquiring than others. Hence it is often best to choose, as the basis of tests, sense-impressions which are unfamiliar to all. The conditions of the tests should be simple, easily intelligible, and, if possible, interesting, so as to induce on the part of the subject a willing coöperation, a natural attitude and a desire to do his best. The tests must occupy as short a time as possible, the apparatus be not easily disarranged by unskilled handling, not too expensive, and, in brief, be practically efficient.

Passing to details the question of method is preëminent; this obviously differs essentially with the various types of tests. For the limits of sense capacity—sensitiveness—as determined by the minimum visible, the minimum audible, the minimum tangible, the question of method is substantially the question of experimental conditions, choice of apparatus and material. For the powers of discerning small differences between similar sense-impressions—sensitivity—the methods are various and have been the subject of considerable controversy, some of which has become of historical interest. The question must be

reconsidered in the light of popular, practical requirements, and, first of all, the necessity of securing a definite, even if not precise, result in a brief time. The method of reproducing a standard sense impression—involving in its calculation of results the determination of the average error—may be highly recommended as satisfying the requirement; but it is unfortunately not readily applicable to all the senses, being most naturally serviceable in the case of those senses which receive impressions quickly and pass readily from one sense impression to another. Ingenuity in arranging the apparatus may do much to minimize this disadvantage. The best substitute for the method of reproduction is the method of selection, the subject selecting one of a given series of sense-impressions arranged in orderly sequence as the equivalent of the standard impression. Of the two other most frequently employed methods, commonly known as the method of right and wrong cases, and of the just observable difference, the former occupies too much time and the latter is too vague in its results. These methods may be variously modified to make them more suitable, as, for instance, the method of arranging a group of sense-impressions in their true order of size or degree, but the difficulty of interpreting the degree of error of the results thus obtained seems to outweigh their other advantages. Sensory tests may involve distinctions of kind as well as of degree, and in these the element of time as well as correctness may be profitably utilized as a criterion of efficiency. This is particularly applicable to distinctions of form and color.

In regard to motor tests, method is again mainly a question of the choice of apparatus and of the groups of muscles to be tested. The qualities of motor response of greatest importance in this connection are strength, which may mean the maximum efficiency at a given moment, or may mean endurance, rapidity of muscular contraction, steadiness or precise voluntary control, accuracy of movement both in itself and in coördination with the eye. The muscle groups to be selected are those which are frequently and familiarly used and easily subject to voluntary control.

The more complex mental tests form a somewhat heterogeneous group. Many of those which are apt to be considered in a popular investigation will involve in various ways a series of more or less complicated distinctions, and of appropriate responses to or modes of indicating the appreciation of such distinctions; while another group arises to test certain phases of memory, or association, or attention or imagination. The former group would be naturally termed reactions,

the time element or alertness furnishing the main test. Typical simple reactions to show the quietness of appreciations of the presence of a stimulus, and a sufficient variety of adaptive reactions to indicate clearly the strength of the powers of distinction and of choice, should form a part of the test. Here, perhaps, more than elsewhere, the adoption of suitable standards is essentially dependent upon coöperative effort. Here, too, the question of apparatus and material is of unusual importance; first, because apparatus for timing is apt to become elaborate, and, second, because the naturalness of the mental processes involved in the test is a function of the details of arrangement. Little can be said as yet of the tests of memory, attention, association, imagination and the like. They are eminently desirable, but in part seem to involve more accurate conditions than it is usually practicable to secure. The most hopeful plan is to have different investigations take up extensive series of tests of special forms of the above powers and let actual experience decide as to their merits.

These remarks are offered by way of a summary discussion of principles underlying the selection of specific tests; their application to the problems of mental anthropometry, now at issue, is a task to the solution of which, it is hoped, that the members of the committee, as well as others, will contribute in the near future.

Professor Baldwin remarked upon the need of giving the tests as psychological a character as possible, thus justifying his preference for the exclusion of such physical tests as 'breathing capacity,' 'height sitting,' etc. He criticised Professor Jastrow's proposals as not giving a separate and prominent place to memory tests. He explained the three methods of testing memory which he developed in an earlier paper (reported to the Association at the New York meeting, December, 1893, by Warren, and published in full in the *PSYCHOLOGICAL REVIEW* for May, 1895).¹ He expressed a preference for the method of 'identification,' as opposed to those of 'selection' and 'reproduction,' on the grounds that reproduction measures or tests expression—which is very complicated—as well as memory-faithfulness, and that selection involved complications arising from contrast, suggestion, etc. The objection that testing by identification involves the use of the

¹ Similar methods were independently expounded by Binet in 1894, in his *Psychologie expérimentale*. Comparing his methods with these, in the *Année Biologique*, I., p. 608, he says that they were "proposés par V. Henri et moi pour la première fois; Baldwin a indiqué des méthodes analogues, d'une manière tout à fait indépendante," evidently overlooking the report of them before this Association in New York.

method of right and wrong cases in computing the results is met when one takes a number of persons together (which need not occupy a longer time); while testing by reproduction, besides being liable to the objection stated above, is open to the difficulty of estimating the errors by the method of mean errors. For example, the calculation of errors in a reproduced series of numbers assumes a criterion as to what sort of a variation in the series shall be called 'one' error. The speaker reported further work in his laboratory on the memory methods. He hoped soon to test a new method suggested by him in 1892, which he called the 'dynamogenic method' ('Proc. London Congress,' p. 51; cf., 'Ment. Devel. in the Child and the Race,' 2d ed., p. 395).

Professor Cattell, chairman of the committee on physical and mental tests of the Association, said that the report of the committee presented at the Boston meeting, being placed at the end of a crowded program, could not be discussed or even read. The committee have consequently not undertaken to prepare a new report for the present meeting, regarding it as more profitable to present before the Association the individual opinions embodied in the report, with a view to securing a full discussion.

The committee agreed to take as the basis of their report a series of tests that could be made on one subject within one hour, and to select the tests and methods with special reference to college students tested in a psychological laboratory. We urged that such tests be made as far as possible in all psychological laboratories, and recommended that a variety of tests and methods be tried, and the results reported to the committee.

The report, already presented and published,¹ shows the individual experience and opinions of the five members of the committee, whence have come to light some agreement and some diversity. The following tests, with some variations in the methods of carrying them out, are recommended unanimously by the committee:—

Preliminary data:—Date of birth; birthplace; birthplace of father; birthplace of mother; occupation (including class in college, or if not a student, the last educational institution attended); occupation of father; any measurements previously made.

Physical measurements: height, weight and size of head.

Keeness of vision.

Color vision.

Keeness of hearing.

¹THE PSYCHOLOGICAL REVIEW, March, 1897; *Science*, February 5, 1897.

Sensitiveness to pain.
Perception of weight or of force of movement.
Dynamometer pressure of right and left hands.
Reaction-time with sound.
Rate of discrimination and movement.
Perception of time.
Memory.
Imagery.

All the members of the committee except Professor Baldwin recommend the following:

Breathing capacity.
Fineness of touch or sensation-areas.
Rapidity of movement.
Visual perception of size.

All except Professor Witmer recommend:

Perception of pitch.

We have consequently, in addition to preliminary records, nineteen tests, all measurements except imagery, recommended with tolerable unanimity. It would require from 30 to 40 minutes to make these, and we have consequently only about 20 minutes of the hour left for additional tests, and I am of the opinion that it is desirable to have that much diversity of work in different laboratories. It seems profitable that at Princeton they should try Ebbinghaus' test of apperception; at Clark, Bergström's card-sorting with practice; at Wisconsin, throwing a marble at a target; at Pennsylvania, will-power and the knee-jerk, and that we at Columbia should measure after-images. I differ from my colleagues on the committee only in so far as I do not place after-images in the series recommended to all laboratories, but reserve it for our private exploitation at Columbia.

In addition to tests recommended by all or nearly all the committee, and those recommended by one member only, there are certain tests recommended by two or three of us, and these deserve special consideration. A test for muscular fatigue is recommended by Professors Baldwin and Witmer and by me. I regard this as a desirable test. Ten pressures on a dynamometer (I prefer the thumb and forefinger) can be registered on a kymograph as quickly as and more accurately than two or three readings can be taken with the hand dynamometer. We secure a strength record subject to mental conditions, and the fatigue curve is typical of the attitude and temperament of the observer. I believe that the only other test recommended by me, and not by at least four members of the committee, is the measurement of height sitting. I do not regard this as important, but it gives a typical indi-

vidual racial distinction, and it is desirable to have a case for the study of the correlation of measurements, where the error of measurement is small. I think it desirable, as recommended by Professors Baldwin and Sanford, to both read and show the numerals in the memory test. We are doing this at Columbia, and the test is consequently now recommended by a majority of the committee.

The tests recommended by two or three members of the committee, but not by me, are as follows: Accuracy of aim in touching a point with the hand is recommended by Professors Baldwin, Sanford and Witmer, and an analogous test, throwing a ball at a target, by Professor Jastrow. At Columbia we also use a form of this test, letting the observer join two points with a pencil, but we do not find it very satisfactory, and I do not include it in my series. It seems to be somewhat difficult to secure uniform method, but I shall be glad to see results when published.

Card-sorting as a measure of quickness of distinction and movement is recommended by Professors Baldwin, Jastrow and Sanford. The difficulty here seems to me to be that we do not know whether we are measuring the natural quickness of the student or how late he stays up at night playing whist or poker. I think that it might be better to use counters or marbles. Should the subject be required to sort one hundred colored balls, it might prove a good test for color discrimination, as well as for quickness. The analogous test of marking objects on a sheet of paper, recommended by all of us, seems to me, however, largely to cover the ground.

Professors Jastrow and Witmer recommend Jastrow's combined test of memory, association and finding time and my test of the accuracy of observations and recollection. The former of these seems to me a good test if used universally and with uniform method, but I think it best to await publication of the results obtained, before recommending it for a short series. My test on observation and recollection suffers also from the artificial character of the questions asked, and further from the difficulty of using the same questions with students who will discuss the matters together before they are all tested. Professors Jastrow and Witmer recommend the accuracy of movements to the right and to the left, but Professor Jastrow does not insist on this as a test of great importance. I am inclined to think that series of movements made simultaneously by the two hands intended to be equal and registered in a kymograph would be a good test, but that it should be further studied before being recommended for this series.

Time will not permit me to discuss the tests recommended by one

member of the committee only, or the variations in method. I should suppose that all the tests recommended in the series proposed, respectively, by Professors Baldwin, Sanford and Witmer, could not be made in one hour, but do not know whether they have tried them or not. For example, Professor Sanford recommends testing perception of pitch to one-half vibrations with tuning-forks, whereas it seems to me that this test alone would require at least one hour. The tests of movement, fatigue and attention proposed by Professor Witmer would also, I should suppose, take an hour. The measurement of motor and sensory reactions recommended by Professor Baldwin seems to me rather a subject for research than for anthropometric tests. But I admit that exactly the tests most interesting to the psychologist are those most difficult to make in three minutes. We can measure the body and make certain tests upon the senses quickly and accurately, but others of our tests, owing to variations in the process and method, scarcely give the real individual aptitude or difference. However, if we test a hundred students, we secure at all events a satisfactory class measure and variation.

HABIT AND ATTENTION.

In the third volume of this REVIEW there appeared a report of some reaction experiments conducted by Mr. A. W. Moore, Mr. J. J. Jegi and myself. We advanced in schematic form a general theory interpretative of the relations of habit and attention, contending that the conception presented, while strikingly confirmed by the outcome of our experiments, afforded for the contradictory results of previous observers a reconciliation more vital and fundamental than any before attempted. The Baldwin and Flournoy theories of reaction both mark distinct advances upon the Leipzig position, which in its extreme form is surely no longer tenable, but neither of them affords a thoroughly satisfactory and complete explanation of the facts as shown under practice, and it is precisely here that the strength of our hypothesis makes itself felt. Owing no doubt to our lack of clearness in exposition, no one of our reviewers, so far as I am aware, appears quite to have caught the force or significance of our contention. Since the publication of our report two of my students¹ have made a series of experiments dealing with the same phenomena, and I take the verifi-

¹ Miss Edith Schwarz and Mr. E. H. Mahood. They made over 4,000 reactions, divided about equally between them.

cation of our position shown by their work as an excuse for adding a few words in further explanation of our view.

Put briefly the doctrine runs as follows: All so-called voluntary action requires for its execution concatenated series of sensory and motor activities. Thus, for example, the placing of a book upon a table requires a complex interlinking of muscular contractions with articular and tactile sensations. Early in childhood the execution of acts of this general character becomes relatively automatic, so that, despite slight variations in individual performances—due to trifling changes in the conditions—the acts obviously belong under the head of habit. Listening, watching and other acts involving strain of attention toward an expected stimulus manifest the presence of habit in characteristic bodily attitudes, which are really sensory-motor coördinations of a less overt type. Movement of the hand in response to a stimulus of sound, as in the reaction experiment, involves accordingly not alone habitual contractions in the muscles of the hand, but also an attitude of listening, which is itself essentially habitual. Moreover, the execution of the act of listening will be vitally influenced by considerations touching the situation as a whole. It will not be done in quite the same way, if the result of the hearing is to be a movement of the foot instead of the hand, and, conversely, the movement of the hand, as an habitual act, will not follow in an identical manner upon a stimulus of sound and one of light. The greater the qualitative divergencies in the nature of the stimuli and responses respectively, the greater the discrepancies in the results. The experiments show this with perfect clearness. In short, the reaction experiment represents an act in which are united two subordinate groups of habitual coördinations, one at the sensory end of the arc and one at the motor end.¹

Now, the function of attention in this process is found in the fact that some of the elements in these interrelated sensory-motor groups are imperfectly connected and require in consequence more accurate coördination. Given a situation where a sensation is to be the signal for a movement, where the getting of the sensation involves a small group of coördinations habitual in type and the movement itself necessitates another such group, the function of attention will be found at that point where the various coördinations comprised in the total sensation-and-movement act are least perfect, least thoroughly habitual.

¹A view of this general character suggests the necessity for considerable modification in the meaning ordinarily given the terms sensory and motor, both as applied to imagery and to the more immediately peripheral processes. Cf. u. Biervliet, *Images sensitives et images motrices*—*Revue Philosophique*, 1897; Professor Dewey, *Reflex Arc Concept*—*PSYCHOLOGICAL REVIEW*, Vol. III.

Interpreted still further in terms of the reaction experiment this will mean that some persons, if asked to perform the reaction, say from sound to hand movement, will find that they naturally and almost inevitably put their attention upon the hand part of the process; that in this manner they react more rapidly and, on the whole, more regularly. With others the exact converse maintains, while not a few find their attention vibrating between the extremes. Attention goes, if one may so express it, to the point of greatest resistance. If we insist that the subject place his attention on some other part of the process, the results show highly irregular reactions. This simply means that with an unpractised subject the coördinations involved in reacting are very unevenly developed, some being more or less perfect and others relatively imperfect. This is exactly what we should anticipate and requires no explanation. If the sensory end of the performance chances to be least reflex and consequently more in need of assistance, removing the attention to the other end of the arc will not only take away the mediation of consciousness from the point where it is most needed, but it will also probably tend to split up the more perfect and approximately unconscious habit at the other end by emphasizing and so dis-coördinating some of its constituents. Thus our point of view, in distinction from a prevalent psychological doctrine, emphasizes the close functional relationship of habit and attention, rather than their polarity and antithetical nature.

If this general statement of the factors involved in reaction be correct, there is obviously no reason why the coördinations employed should be insusceptible of the same kind of training which characterizes all growth in habit. Accordingly a person manifesting at the outset marked subjective preference for the motor form of reaction, with relatively rapid and regular results, ought to be able, other things being equal, to develop a similar capacity for reaction in the sensory form, so that eventually he should minimize, if not obliterate, the differences in the outcome of the two forms.¹ I say, 'other things being equal' because we shall here, as elsewhere, meet with limitations in the degree of facility with which new coördinations are adopted. Very few persons, for instance, are capable of any high development in the mere muscular technique of piano playing, and,

¹ I disregard the various modes of reaction included under the general terms sensory and motor respectively, not because I distrust the genuineness or value of the distinctions concerned, but simply because their truth or falsity leaves wholly unaffected the point I am at present urging. As a matter of fact our original article contains mention of several clearly differentiated forms and Baldwin and Flournoy contribute a much fuller list.

while discipline will generally do much, its limits are sooner or later inevitably encountered. As the reaction-times represented by the two forms draw together under practice, both of them should become more rapid and more regular. Personal peculiarities will probably tend to render one form permanently more rapid, but there is no reason why this form should be the same for all individuals, and in cases at all susceptible to drill the differences ought to be relatively small anyhow. On the other hand, our position offers, along the lines already indicated, a thoroughly cogent account of those cases, emphasized by the earlier investigators, where the reaction-times of the two forms show under practice little, if any, tendency to approximation. I am strongly of the opinion that these cases will be found relatively infrequent, if the experimentation is (1) free from strong preconception on the part of the subjects regarding the outcome and (2) if it is carried on long enough to pass through certain well-marked transitional stages.

A few citations from the experiments, which afford me the excuse for this statement, will illustrate the points I have been presenting. Early in the course of experimentation one of the subjects shows, as a result of 80 (ear-hand) reactions taken on one day in the sensory form, an average time of 198σ with a variation of 38σ ; 96 experiments taken under similar conditions a month later show the average time reduced to 138σ with a variation of 27σ . In the same period the motor form, starting (85 experiments) with an average time of 158σ and a variation of $39\sigma +$, has come down (52 experiments) to 132σ with a variation of $19\sigma +$. This shows the development progressing, but not yet complete. Both forms are growing faster, and the sensory, which was originally the slower, is gradually approximating the time of the motor, while the variation for both forms dwindles. The other subject shows in the first half of 674 (ear-hand) reactions taken in the sensory form an average time of 176σ with a variation of 21σ . In the second half the figures are 161σ and 16σ . Out of 718 (ear-hand) motor reactions the first half gives the figures 153σ and 15σ , the second half shows a decrease to 135σ and 13σ . 387 ear-lip reactions taken in the sensory form, and after benefit of the practice from the tests just mentioned, show in the first half 127σ and 14σ ; in the second half 119σ and 11σ . 456 motor reactions of the same kind give in the first half 115σ and 9σ , in the second half 112σ and 8σ . In the second subject's ear-hand reactions the increasing rapidity of both sensory and motor forms is clearly shown, although the approximation of the one to the other has not yet set in. In the same subject's ear-lip reactions, however, this approximation is already obvious.

It must not be supposed that the approximation of the reaction-times means a complete merging of one form in the other. Not only do the mean variations show a real difference, but also the unanimous verdict of the subjects is, that under practice the differentiation of the sensory and motor forms is increasingly clear and easy, the varying direction of attention being perfectly unmistakable, however ambiguous it may have been at the outset. As the various coördinations involved approach completeness and perfection, it becomes more and more a matter of indifference where the attention is directed. The environment is probably never so fixed and unchanging—to say nothing of the organism itself—that the coördinations in response to it are not capable of being slightly bettered by the presence of attention, otherwise the whole process would ultimately become automatic and essentially unconscious. The changing situation, both without and within, alone prevents such an outcome. At all events, this appears to be the case so far as concerns those particular coördinations involved in the reaction experiment.

JAMES ROWLAND ANGELL.

UNIVERSITY OF CHICAGO.

THE 'TRANS-SUBJECTIVE' AS PSYCHOLOGICAL FACT.

Truth, even when unpopular or unrecognized, is still truth. The occasion for this remark will, it is hoped, be apparent as we go on.

No work published hitherto has, to my knowledge, made the thorough-going attempt, presented in Professor Ladd's recently published '*Philosophy of Knowledge*,' to develop a theory of cognition based on the renewed assertion and defence of a neglected and even denied postulate. That truth is the so-called implication of the trans-subjective reference in every genuine act of complete knowledge. The purpose of the author, announced in the preface, 'to make epistemology vital,' must, of course, depend for its success or failure, as has been foreseen, not only on the brilliant and sometimes startling insights, the connected and calm current of the thought, the over-flowing earnestness of conviction, apparent on each page, but also, if not chiefly, upon the validity of that realistic view of consciousness which makes every completed act of cognition 'take hold' of the trans-subjective.

In his apparently hasty review of this, in the sense above defined,

'pioneer' work, which appeared in the pages of this REVIEW,¹ I was pained to find that Dr. Miller had gone out of his way to cast a sneer at the author for announcing his claim to the position of 'clearer of obstacles.' Whether Professor Ladd is guilty of the set of feelings attributed to him, among which may be mentioned literary injustice and 'critical acerbity,' must be left to his readers to decide; for my own part, I can discover not the slightest trace of them. But passing this over, we come to a more remarkable feature of this review, *i. e.*, Dr. Miller's assertion² that Professor Ladd has nothing but 'asseverations' for his claim that knowledge, as a complex psychological fact, involves the trans-subjective. Dr. Miller goes so far as to declare: "The phenomenist might simply deny the alleged trans-subjective intention, the implication of the 'transcendent' in knowledge—as the present writer at all events does deny it—might declare that there is no such psychological fact; and Dr. Ladd could have nothing for it but asseverations." It is remarkable enough that this crude kind of phenomenalism should satisfy Dr. Miller; but it is astonishing indeed that he should also try thus to plant Professor Ladd in the same position; for the last description perhaps that could be applied to 'The Philosophy of Knowledge,' as that work implicates this disputed postulate, is the one employed by Dr. Miller for that book.

Dr. Miller appeals to facts; to facts, then, let us go. Is the trans-subjective a psychological fact? a datum of cognition? is this implication an unmeaning and illogical creation of the medievalists? a postulate having no ontological validity? These are some of the questions suggested for reflection in this connection. Or, more briefly and positively, we may ask, Does human knowledge, psychologically viewed, envisage not only an activity of the self, but also a conviction of extra-mental reality, the type of which may be found in self-consciousness? In answer to these questions I will briefly present three instances or cases where our ordinary cognitive acts cannot be fully accounted for without the trans-subjective reference.

Take the complex phenomenon called 'awareness of an object.' This can be analyzed into three parts, which, for brevity's sake, may be designated stimulus, reaction and relation; and I presume that, in the light of physiological psychology, we cannot get along, so to speak, with fewer. Now, the cognitive aspect of sensation, which is our immediate concern, necessarily involves (in order that it may be cognitive): first, the knowledge of a real cause of the state called 'awareness

¹ November, 1897.

² Made on p. 650 of this REVIEW.

of an object; ' second, the recognition of the state as *my* state; and third, the differentiation of the cause from my state and yet the implication of the activity of the self in and with the real cause. It is nothing but the rapidity, intensity and changing quality of our sensations that prevents the immediate conviction of the truth of this analysis. As I understand the phenomenalist (and with him Dr. Miller classifies himself) it is the last factor in our sensuous cognitions that is omitted in their analysis, and flatly denied; *i. e.*, they doubt and deny the implication of the transcendent cause (*causa sui*) in our knowledge of the object, so that all that is left is the object as state in my consciousness; but in this case, as will be seen, the richness and fulness of our cognitive experiences are suddenly deprived of their 'vital' factor. If our analysis, however, be adequate to the facts presented to reflective thought, there is thus postulated, even in our elementary and primary cognitions, though in an inchoate way, and certainly not in the form known to developed self-consciousness, the other-than-self which has the cause of its existence in itself. The question is whether any other analysis does full justice to all the facts of consciousness.

It is acknowledged, of course, that standing by itself alone, thus early in cognitive life, this case of the transcendent implication in knowledge is a very poverty-stricken one. But the essential thing is to recognize and show, if possible, that it is not a manufactured article, a speculative necessity merely, a support for ignorance; but a genuine datum of cognition, in some real sense the chief datum where the 'awareness of an object' is in question. That it is genuine may be further shown by attending to the psychological function upon which it rests. How does the mind get its objects? It is not enough, it seems to me, to say, as Professor Bowne does:¹ "The constitutive action of thought penetrates even into sensation as an articulate experience; and sensation becomes anything for thought only through the action of thought itself." This is true enough and unanswerable from the phenomenalist's standpoint. But is there nothing else involved in the process than 'the constitutive action of thought'? The question is: How does the mind *get* its objects? The full answer to this fundamental inquiry cannot ignore the relation of interaction constantly going on between the self-active ego, and the active cause of its states called the object. So far as the latter is concerned it is plainly not enough to say, it is given. This is to beg the whole psychological question. A brief remark on the so-called 'process of diremption' may serve to illustrate my meaning. This is the crucial process whereby our

¹ 'Theory of Thought and Knowledge,' p. 38.

primitive intellective experience becomes differentiated into a body, a non-ego, and a mind, an ego. Now this experience is possible solely because all our intellectual functions are discriminating and assimilative. Without entering now into the influence of heredity in predisposing the mental mechanism to make quick or slow reactions, it is plain that the act of discrimination itself is not evolved; for that would, if claimed, involve the absurdity of getting from nature what makes nature as a discriminated reality possible.

A similar remark applies to the assimilative activity. Now it is in obedience to these regular modes of the mind's behavior that the undifferentiated continuum of impressions, emanating from the causal object and raining down unceasingly upon us in every moment of our life, becomes broken up in an object and a mind that knows it.¹ Of course the gain, so to speak, in this process, is on the side of the mind, and it is the mind that is awakened and imparts its order and law to our early experiences of a noetic sort; but what is essential for our present purpose is to note that the whole 'diremptive process' rests upon the implicit connection, active in every cognitive state, and generally unrecognized because the process is so subtle and quick, namely, the vital connection of the discriminated groups (body *and* mind) in the assimilating and self-identifying mind. We shall recur to the distinction which the reader will readily detect in this statement between consciousness and self-consciousness. It is enough if we observe that in this elementary experience there is 'awareness of an object' discriminated from the self, the knowledge of which involves the capacity of the mind to refer those changing impressions and sensations it experiences to the object as '*causa sui*.' If this be denied, we are reduced to the phenomenalist's position which, disregarding the active aspect of mental life, especially as illustrated in these processes, resolves the terms of our ordinary language ('object,' 'self,' etc.) into mere '*schein*'; thus obeying a tendency, inherent in its standpoint, the outcome of which is the deduction of knowledge from that which has no active participation therein. The phenomenalist would dispense with the extra-mental reality which, to use Professor James' expressive term, provides the 'prick' of our sensuous knowledge.

Closely connected with this is the question, What is the object known in perception? Controversy has raged with unusual heat, and I presume will continue to rage, on this problem. Here we are concerned with psychological facts verifiable by introspection and re-

¹ Cf. 'Ladd's Psychology, Descriptive and Explanatory,' Ch. 15 and 16; 'Philosophy of Mind,' p. 244 ff.

flection. Two remarks may be permitted: it is generally overlooked that in the experience we have been analyzing it is not all reality that is in view, but some particular object having definite presentable constituents, and whose changing states we are made aware of in our living intercourse with it. The object known in perception is always the here and now present object—the book, the table, the pen, the hand that writes, the ‘flower in the crannied wall,’ etc. If our experiences are not thus *referable* to an ordered system of objects upon which we have learned to rely, the presupposition of all science is taken away. Again, the object of which I am aware in sense-perception must, in order to be an object to consciousness, be clearly discriminated as the cause of those states the knowledge of which involves the act of reference for which we contend. Thus ‘object’ means particular object in concrete experience, known, as the subject and cause of its own state in self-consciousness. We contend, at this stage of the matter, for no more or less than this, and for this much we believe we have verifiable facts. It is not clear, to the ‘child or the rustic,’ whether the object, whose known states or changes I refer to their transcendent cause or ground, is or is not a sort of *tertium quid* between the ego and the extramental object; but is there not a real sense, a trans-psychocial, in which the trained observer may see what is not obvious to the untrained? But the difference is not in the essential content of their experiences. Each is conscious of the same essential content. Each envisages in sense-perception an extended and extramental reality, the knowledge of which depends upon the discriminative and trans-subjective function which we have already analyzed. If the facts presented are not hallucinations, pure figments, or worse, consciousness here presents us with an instance of the experience so categorically denied by the phenomenalist.

Another instance may be briefly presented, namely, the trans-subjective intention as involved in the inferential function of the judgment, and especially the question, implicit in every act of knowledge, of the *validity* of logical processes. The first is, of course, a purely psychological, the second a logical, question; yet they cannot be safely separated. Now, to judge is to relate things according to their conceptional elements. As such all thinking is judging, and both thinking and judging are essential functions of knowledge. Judgment, then, psychologically regarded, is the predication of thought to things. This function, however, also rests upon the same presuppositions which we have found to underlie the experience called ‘awareness of an object,’ namely, the abiding conviction that, independent of

our thinking, there *exists* an object so changeable within limits as to be known, and, second, that the knowledge of that object involves the postulate of the constant interaction of the self and its transcendent object; only the area of definable objects is now indefinitely enlarged. We shall never get at the real nature of prediction unless these psychological experiences are borne in mind, especially as this problem turns on the question whether, or no, every judgment involves an objective element, *i. e.*, a trans-subjective implication. Take the judgment: "the child's milk is hot." In this judgment there is plainly postulated a relation *between* the child and the object, milk. The prediction 'hot' is made on the implicit assumption of this dualism. Is there anything further? Is 'hot' a quality of the sensation known through the organs of the complex muscular temperature, and taste senses, and nothing more? Is the mind conscious only of its sensations when using the predictive term '*is* hot?' Let us distinguish here between the general question of inference, which is generally agreed to be a function of judgment, and the peculiar characteristic of *objectivity* which all our judgments among *things* possess. I contend that the living connection, the vital part, of judgment is our consciousness of an implicit power to apply, transitively, thoughts to things, a phenomenon which cannot be better described than as the judgment of the essential identity of the matter of our objective cognitive acts. And, when we turn to the logical form of the question, the situation, so far as it implicates the transcendent, is not greatly changed; for no hard-and-fast line can be drawn between psychological and logical judgments, the difference merely being that logical judgments depend upon a more developed condition of the conceptual activity than psychological. And it becomes recognized very soon in reflective experience that the logical relations among concepts, regarded content-wise, are far from being clearly and immediately accepted as real. That this is so we can readily show by substituting thing-relations for thought-relations in any supposed categorical judgment of the logical sort and note the change in consciousness: *i. e.*, many logical judgments have for their subject-matter what is unreal and even impossible merely as *thought*. This condition of things forces the mind, which never retreats from its conviction that the vital connection between the subject and object is intact, to question the validity of its logical processes, and seek for the factor, or experience, that makes a logical judgment determinative of reality and, therefore, of knowledge. Is there such a factor verifiable or recognizable in consciousness? By 'fact' or 'factor' I do not mean an experience limited by the present

content of consciousness, or by any part of the total content assumed to be covered by the word 'experience.' I mean, if the term may be permitted, the possible-for-consciousness, as well as the actual-in-consciousness; it means, therefore, for the time being at least, in the period of doubt, the other-than-my-present-consciousness; the ideal which has reality and cause *per se*, which, without the act of self-transcendence, the trans-subjective leap is an empty category and without which no valid judgment of reality ever does take place. You may call this factor 'belief,' or 'faith,' but two things are plain about it: one is that every cognitive act rests upon it, every logical judgment passes over into this more ultimate question of the validity, *i. e.*, the truth, of our cognitive processes; the second thing is that all efforts to exclude it from consideration have failed to destroy its indomitable presence. To our moorings, in reality, the self-realizing mind must be ever anxious to return, from the conceptual and abstract formalism of mechanical logic. And this process, as the history of the transition from medieval logic-chopping to the Baconian method shows, involves not only a change of attitude, but a recognition of the vital connection between the self-known mind and its extra-mental object, as well. Enough, perhaps, has been said to show that gratuitous scepticism of the transcendent implication in knowledge is an impossible position, when we have regard to our logical judgments; for blind materialism or solipsistic idealism is the only escape from denial.

We have but to add that the categories, according to which the mind works out its logical construction of the word, already postulate that identity in difference, which the phenomenalist seems disposed to call in question. Knowledge is not a projection of our conceptions upon the extra-mental reality with which it is in living contact. The 'trans-subjective' rests upon a psychological activity, which in turn depends upon the essential identity of thought and thing and their adjustment in cognition. A knowledge of things would otherwise be practically impossible. That our categories are valid in reality implies not only the psychological possibility of making the application, but also and equally upon the self-identity, so to speak, of the particular object, whose nature is in question.

This brings us to a third instance of the trans-subjective as psychological fact. It has been hinted at before that there is a distinction, for the writer, between consciousness and self-consciousness; and of self-consciousness there are stages of development. But even inchoate consciousness of self more or less explicitly recognizes the fundamental distinction between mind and body. Tennyson tells us that

even the sucking infant never 'confounds itself with the circle of the breast.' The type of experience here presented may be expressed: *I* think, *I* act. This deliverance of consciousness is immediate, and forms the raw material, as it were, which results, upon this occasion of the operation of the principles of mental development, in the more developed consciousness of self. Professor Edward Caird has clearly stated the case in the words: "In determining himself as a self, the individual *at the same time* excludes from himself every other thing or being and determines them as external objects." The question is whether any analysis of this experience covers the facts, if it disregards, ignores, or underestimates the presence in consciousness of the distinction of the self from the object, not merely as a 'content-fragment,' but as implicating the transcendent. I contend that analysis and introspection render a negative reply to this question exceedingly doubtful, and on the positive side imply the derivation of the *self* from unconsciousness, and the resolution of the *object* into a subjective phenomenon. Philosophically, materialism is the consequence in one case, in the other solipsism. But psychologically these are impossible positions, for even the inchoate consciousness of self cannot be evolved or explained according to the laws of association and suggestion, for, before these processes have had any extensive area of effective operation, before a discriminated field of objects has been built up, the elementary perception of a contiguous series or manifold of *unanalyzed* objects, set over against the mind, is already implied *as a condition* of the discrimination. The power to discriminate cannot be derived, evolved, or imparted by the thing or object discriminated. The 'extra-mental' cannot, therefore, be an inference from a subjective state. The distinction between consciousness and self-consciousness shows the impossibility of this, if indeed we are not too empty of all reality, and causality, if not of permanence, the whole field of knowledge. On the contrary, the experience known as self-consciousness implies a direct knowledge of the ego as a real existence, as a unitary being, and as both possible and actual in every psychosis of a noetic sort. If these be the facts of the case, the trans-subjective is implied in this ultimate experience. It is no argument against this position to say that the self-known self of experience is a growth. The much-abused child and rustic are only partial evidence and, so far as I am aware, are never used for more philosophical purposes; but they are still indubitable evidence and guides of our more developed thought. Of course, there are no data, capable of sensuous examination or logical manipulation, marking off the area of consciousness.

and self-consciousness; but this does not vitiate the general soundness of our representation that the distinction, implicit and inchoate at first, grows clear under the complex sensational and volitional experiences which enter into it and into all knowledge.

The connection of this complex with the cases already presented illustrates the vital bond that binds together all the isolated experiences of the mind into a living unity. For nothing essentially new, as concerns the immanence of the transcendent in cognition, is added to the fundamental conviction, which is implicit even in sense-perception, that all experience rests upon a real dualism, between the elements of which there is constant interaction. The development of knowledge presents nothing which psychological science is obliged to regard as, in any sense, a violation of these types. The phenomenalist is free, of course, to deny the facts, but the onus probandi rests with him, and from this there is no escape except the attempt to include our transpsychical intention in more genetic conceptions. Against this purpose we have no immediate objection so long as it is not deprived of reality; but we have already given our conviction that phenomenism is obliged, by its inherent tendency, to regard our contention as unnecessarily complicating the problem. The immediate question, however, is one of fact, and this the development of self-consciousness, supported by the experience called 'awareness of an object,' and by the inferential validity of our logical judgments, clearly shows to be reasonable. The self, whose essence is known, especially, in volition, presents us with the most fundamental type of reality, the knowledge of which consists in the imputation to the object of consciousness a similar unitary and causal power. If this can be done, in any way appreciable by common sense, without the implication for which we contend, and without violating those real distinctions upon which science and metaphysics depend, the way has never yet been made clear since the time when speculation began. Another consideration, the value of which has not been appreciated by phenomenism, is, that the self is a teleological conception; the tidal waves of experience, when the summation of many often conflicting lines of thought seems to bring us to a more satisfactory condition, for our knowledge, rest upon this conception. The incessant activity of the self-conscious mind follows a method, which expresses the reality of that mind progressively. The differentiation, which is incident upon these epochs in our experience, of course involves the philosophical doubt as to the real nature of the extra-personal object; a doubt that can be laid to rest only as the ideals and ends of the self-conscious reason are analog-

ically applied to the object by the doubting mind, which, even in doubting, postulates that self-transcending act which accomplishes what is known as knowledge. Space will not permit us to enter into this large subject. But popular thought, especially on scientific subjects, is abundant proof of the impossibility of denying the trans-subjective implication. For example, one of the burning questions is connected with the ethical and æsthetic implications of naturalism. Now, the possibility of regarding nature as an ethical entity clearly depends upon the philosophical meaning of the word 'nature,' and on the psychological facts connected with the activity and processes of the only being which is known directly by the mind, *i. e.*, the self-known self of our daily experience. The teleology of self is immediately known. Whether 'nature' is, therefore, teleological, or not, will ultimately depend upon the defensibility of the fact for which we have been contending. As an extra-mental object nature does not tell us; 'red in tooth and claw,' there is much that points to the pessimistic conclusion supporting naturalistic pantheism. But this problem can be resolved only as the reality of the object is more and more investigated according to the standard provided by the living soul in its total progressive activities. In the very heart of this work, so subtle and quick as to be unrecognized, there is going on an incessant intercourse, which, though subtle and quick, recognizes the claims of each series and system of reality, and of each individual object as possessing an essential self-identity which forbids its departure from the normal course of its activity. If that individual object, standing thus in a system of reality, is a subject of ethical and æsthetic, as well as physical changes, it can be known only as we know ourself as subjects of similar changes and apply, trans-subjectively, the interpretations of reality to it.

The full descriptive history of this phenomenon it is impossible to present in the limits at our disposal. The essential truth we have contended for may, however, be seen in the types of normal experience which have been presented. Other cases will readily present themselves, especially when we have regard to the phenomena of abnormal psychology; but they may all be regarded as the extension of these known cases, and the laws which regulate their operation. From the psychological standpoint, then, it would seem that something more than 'asseverations' may be brought forward for Professor Ladd's contention. A remark or two, from the logical and metaphysical points of view, may be permitted, especially as these culture interests involve psychological facts and processes.

Dr. Miller has not only denied the trans-subjective as psychological fact, but charged Professor Ladd with attaching no logical meaning to it. He declares that it rests on a self-contradiction; namely, a being "cannot resemble another in its numerical identity and by its own being convey the existence of something else."¹ This conception of the fact in question is unpsychological. It is not a fact at all, but a hypostatized entity torn from the living contents of self-conscious experience. We have no knowledge of such a process. The trans-subjective implication does not create the extra-mental as the phenomenalist supposes; this would imply that the object is nothing but a 'content-fragment' of consciousness. The cases we have presented show that sense-perception, taking all practical uses into consideration, objective predication and self consciousness, develop in the unity of a complex growth. There is no isolated content-fragment called the trans-subjective, capable of separate analysis and elevation to the dignity of a logical or metaphysical postulate. But it is none the less a vital factor in cognition, upon which both logical and metaphysical activity, as they enter into knowledge, depend.

And from the logical standpoint also no valid answer is constituted from the conception of numerical identity, for this is not a logical concept, except within certain well-defined limitations. It is inapplicable to any fact beyond the particular instance we may have in hand, and only so long as we choose to maintain the identity. There is no logical reason why we may not regard the self-identical being as different, so long as we honestly announce it as such. Logic has nothing whatever to do with this change, or the introduction of a new predicate. That is a matter of psychological analysis and truth. If this also be denied, the whole *practical* importance of a fact will be lost. For logic, as for psychology, the supreme question is not that of consistency, but the question of realizable conditions or truth. Dr. Miller's logical nominalism is the immediate result of the attempted divorce of the living connections and variety of our concrete experiences. There is only one way of escaping, it would seem, from this predicament, and that is to hold with Lotze,² that from the law of Identity nothing can follow which qualifies the reality of the being and to recognize that the concrete facts of our experience and knowledge make being and change mutually inclusive conceptual facts of the cosmos.

Finally, the empirical consciousness of the trans-subjective enters into our metaphysical view of the implications of knowledge. If there

¹ P. 652 of the REVIEW.

² Metaph., § 39; cf. Logic, § 55.

were time it might be shown that it is not alone the consciousness of the child and the rustic, but also that which has been rendered scientific in such ways as a theory of evolution that involves the experience for which we have contended. There are few who will to-day hand in their assent to a purely determined view of knowledge, either subjectively or objectively, but the surprising feature of the situation is that so few see that the necessary correlate is the admission of the mind's power to transcend its crude dualism, its cold Spinozism, and seize the causal subject in its essence. Evolution is still the evolution of some thing or mind, and of this we have, and can have, no knowledge, if the reality of that thing or mind is separated from the primary as well as the most highly cultivated and complex psychological experiences in a permanent way. Essentially there is nothing unrelated to reason; but we have to get our knowledge by degrees; and this is possible only as the mind, possessing reality, is able to explore the ocean of being which lies spread out before us, as in some real sense, reflective of a life which, far transcending ours, is, nevertheless, identical with that which we ourselves experience.

Phenomenalism, therefore, must not simply deny an unpopular or unrecognized truth, but go to work and *disprove* its right to existence. Until it has been argued out of existence it is still truth, and will survive the shocks of debate. Meanwhile, awaiting the treatment, it will continue to provide ground and cause to our higher ethical and æsthetical experiences, as well as to those more primary questions. Phenomenalism, with its implicit agnosticism, too, will continue, and mete out denial to these claims. I hesitate to classify Dr. Miller among this class of thinkers, therefore I can say without the suspicion of offense that I regard phenomenalism as the worst of abstractions and the veriest cant of current philosophy.

HENRY DAVIES.

CONSCIOUSNESS UNDER NITROUS OXIDE.

An English correspondent sends me the following account of his subjective experiences during nitrous-oxide intoxication. I place it (with his permission) on record in the *PSYCHOLOGICAL REVIEW*. Normal human consciousness is only a narrow extract from a great sea of possible human consciousness, of whose limits we know nothing, but of the nature of portions of which such documents as the following may help to inform us. It were greatly to be wished that they might be multiplied.

W. J.

The note in your book, entitled 'The Will to Believe,' upon the above subject, recalls to my mind a strange experience which I had in June, 1895, while still an undergraduate at Oxford.

I had been studying philosophy, and had about as much acquaintance with it as a man gets in two years, who has a good deal of natural interest in abstract speculation, but very little natural talent for it. The ideas of Hegel, though exercising a tolerable fascination over my mind, were only known to me at second or third hand, through English and Scotch writers and casual conversation.

One morning in June, 1895, or certainly not later than the end of May, I went round to a dentist's opposite Balliol College, to have a tooth out. I had never 'taken gas' before, and never have since. My experience was, as accurately as I can remember it at this distance of time, as follows:

Either of set purpose, or to distract my mind from the intensely uncomfortable process of 'going off,' I determined to observe very carefully the changes in my conscious states.

What happened, I found, was that the contents of consciousness, the feelings, gradually became reduced, till I came down nearly, though not quite, to the bare uncolored fact of consciousness of existence almost divorced from sensation. By this time, of course, I was hardly in a position to observe accurately, but when I came afterwards to think the matter over, it seemed that I had spent an absurdly long time in this state, and then suddenly, when I was hoping for it, but least expecting it, had 'gone out,' like a snuffed candle.

The next experience I became aware of, who shall relate! my God! *I knew everything!* A vast inrush of obvious and absolutely satisfying solutions to all possible problems overwhelmed my entire being, and an all-embracing unification of hitherto contending and apparently diverse aspects of truth took possession of my soul by force. The odd thing, and one that sent a ripple of merriment through my consciousness, was that I seemed to have reconciled Hegelianism itself with all other schools of philosophy in some higher synthesis. The biter bit!

Then, in a flash, this state of intellectual ecstasy was succeeded by one that I shall never forget, because it was still more novel to me than the other—I mean a state of moral ecstasy. I was seized with an immense yearning to take back this truth to the feeble, sorrowing, struggling world in which I had lived. I pictured to myself with justifiable pride how they could not fail to recognize it as being the real truth when they heard it, and I saw that previous prophets had

been rejected only because the truths they brought were partial and on that account not convincing. I had a balm for all hurts, and the prospect of how entire humanity would crowd around to bless the bringer nearly intoxicated me. But I thought I was dying and should not be able to tell them. I had never cared much for life, but it was then that I prayed and strove to live for the world's sake, as I had never prayed and striven before. It seemed in vain, however, that I battled for life, and I was just resigning myself to extinction when an immense sense of relief and of some obstacle having given way broke in upon me. This was, of course, succeeded by another fit of philanthropic ecstasy. Five or ten seconds more, and I should be able to speak, and the world would *really* be redeemed, whether I lived on or not. It was a moment of the supremest bliss, exceeding those former ones. Suddenly I saw standing on a little pink stage a little pink man with a kindly face which I seemed to recognize. Who could it be? Then, as the little pink man grew rapidly larger and less pink, and I steamed into the position of normal consciousness (for that was the sensation) I heard a voice, apparently not that of the little pink man, but coming from some one out of my range of vision, say: "That would have been a tough job without the elevator." These words gave me power to speak out, and I shouted aloud: "That would have been a tough job without the elevator; I've found out some metaphysics!" Hardly had I said the words, however, than they mocked me. The truth had evaporated, like a forgotten dream, and left me with half-formed phrases on my lips and an ashen-gray delight in my heart. The dentist asked me whether I wasn't suffering from a sluggish liver, and the little pink man, the doctor, recommended me to go away for a change of air. Shades of the prison-house have since closed about me, and Professor Caird still reigns unchallenged at Balliol.

PSYCHOLOGICAL LITERATURE.

Man's Place in the Cosmos and other Essays. By ANDREW SETH, M.A., LL.D., Professor of Logic and Metaphysics in the University of Edinburgh. New York, Charles Scribner's Sons. 1897. Pp. viii+308.

This volume is composed of four extended and controversial book reviews, two of which may fairly be called essays, and an inaugural lecture delivered on occasion of Professor Seth's assumption of his present chair at Edinburgh. These pieces afford a graceful and, in a sense, lucid expression of a vein of philosophic thought in which both Reid and Lotze would find much of their own. No way of thinking in systematic philosophy is at present much more familiar, and it would be speaking loosely to say that a new expression was called for. No exceptional force or freshness in the chief expositions demands our notice, and little ingenuity or originality makes itself evident by the way. Dialectically the polemic is less telling, though it is perhaps more specific, than many others that have gone before. The author announces that his tilt is against "mechanism in physiology, 'presentationism' in psychology, materialism and sheer pantheism in philosophy." If any of the somewhat hardened followers of these dubious doctrines (save materialism, which in strictness I fancy has no philosophic followers at all) are shaken or even bruised by the decorous blows, largely of epithet and grave assertion, that Mr. Seth rains down upon them, they are less 'proper men' than one takes them for. Some of the 'presentationists' in particular seem palpably tough and dare-devil campaigners, formidable at the close thrust of argument, and to send out Reid and Lotze once more to the attack, and Mr. Seth to lead them, will demoralize such an enemy only with genial diversion. Far better leave them to the penalty of their name. One dislikes to see Mr. Seth in an unedifying plight. There is a sustained refinement and 'gentleness untired' in his pages (save for one melancholy lapse) that much commends him to his reader, who perchance has come to him black-and-blue from the drastic hands of Mr. F. H. Bradley. Here one glides equably along, secure from the shocks even of novel thought or expression, and forming no anticipations that are destined to be disappointed. One has a sense at the close that a

typical conservatism has expressed itself in a somewhat impotent but a fastidious and harmonious form.

It is interesting to mark the contact of such a conservatism with another equally fastidious and harmonious in its expression, but incomparably more powerful. The closing piece is devoted to a friendly interpretation of Mr. Balfour's 'Foundations of Belief.' Mr. Seth's aim is to show the alienated philosophic critics that Mr. Balfour has outgrown the subversive scepticism of his earlier 'Defense of Philosophic Doubt' and that, taking the word in a large sense, he bases all in his later book upon Reason. The interpreter deals with his text in considerate elder-brotherly style, softening and mellowing Mr. Balfour's audacities of expression, deprecating his 'love of mischief,' his 'unfortunate terminology,' his 'unguarded' passages which "have given offense in quarters where, it is certain, they were never intended to do so"; earnestly drawing attention to the 'difference of tone' about reason in this later work. Undoubtedly in the stress he lays upon its positive thesis, Mr. Seth does service to a book of splendid originality and force which has been misconceived and mishandled by eminent critics in a manner that can hardly have heightened its author's very moderate respect for the philosophic authorities of the time. But in asserting an inconsistency between the 'Philosophic Doubt' and the 'Foundations of Belief,' and in offering, as it were, to throw over the former as a prey to the rationalist critic if he will but do homage to the latter, Mr. Seth fails of insight, one ventures to think, into his author's mind. The later work adds indeed a strikingly new argument to the earlier, but in no substantial degree traverses its positions. Even in the later the *ultimate* appeal (for our acceptance of science at all, for instance) is to impulse and not to reason. And it is instructive to note how the workmanship of a mind at once of fine logical scruples and firm speculative nerve loses its sharp-cut outline when reproduced in a book whose academic correctness of form covers vagueness and flatness of thought.

The paper dealing with Huxley's Romanes lecture on 'Evolution and Ethics' sets clearly forth that since to rationalize things completely we require not only satisfactory causes, but satisfactory ends, we have the warrant of reason for assuming the cosmos, in which Huxley found no evidence of an ethical character, to be subject in the last resort to a purpose supremely ethical. The prior assumption, as to the teleological demands of reason, is too obvious for remark. The inaugural lecture is not so much on 'Man's Place in the Cosmos' as on the place of the different branches of philosophy in university teaching. The extended essay on Mr. Bradley's 'Appearance and Reality' is perhaps

the most solid in the book. It disputes with clearness and plausibility that author's violent dealings with appearance and his peculiar species of pantheism, but the argument, after Mr. Seth's fashion, is hardly brought to close quarters enough to take decisive effect upon so subtle and nimble an antagonist.

In the pages of THE PSYCHOLOGICAL REVIEW the essay on "The 'New' Psychology and Automatism" calls for especial notice. It is an elaborate attack upon what are accounted the degrading doctrines of sensationalism and automatism as set forth by Professor Münsterberg. In it are gathered together in conveniently brief compass most of the commonplaces of retort on this head with which students of the history of philosophy since Hobbes are familiar. The chief arguments are not of a varied character. On Dr. Münsterberg's analysis of the will, 'there is no active subject at all.' Again: on his theory 'there is an elimination of all causality; sequent ideas are all.' Again: 'In the very act of emphasizing movement and the dynamic aspect of ideas, he eliminates altogether the notion of action or activity.' Again: 'The recognition of the subject becomes an empty acknowledgment. It is entirely denuded of activity, all action being refunded into the play of presentations.' Again: the 'presentationist' resolves "volition into a sequence of presentation; first an idea, then a perception (as we have seen Münsterberg put it), but no intervening fiat, no power, no real action, nothing corresponding to what we mean by volition." Again: "Volition is the action of a subject, and as such cannot be phenomenalized." Again: "But does it not require some effrontery to offer us these antecedent, concomitant, and sequent ideas as an account of the *volition* itself?" Quotations of this order, in which the words 'impudent' and 'brazen retort' mark further lapses from our author's customary good taste, might be multiplied almost at pleasure.

Against commentary like this, protest should be made in the name of the ethics of controversy. To charge psychologists (especially in a popular review, where this article originally appeared) with denying 'the active subject', with suppressing 'mental activity', with leaving 'nothing corresponding to what we mean by volition', is to commit a somewhat gross misrepresentation; and to cast at them such epithets as 'impudence', 'effrontery' and 'brazenness' for so doing is seriously to aggravate the offense. The truth is that they do not deny or suppress these manifest features of conscious life; they merely profess to analyze them. What instructed student is not weary of hearing that the idealist 'denies' matter, the determinist 'denies' choice, the nominalist 'denies' generic ideas? In point of fact in all these and

many kindred cases the supposed paradoxical denier begins with acknowledging the plain facts in question and goes on to offer an analysis of what they are. It might as well be declared that Professor James 'effaces' (in one of Mr. Seth's words) the emotions. It is time for all who would speak in serious psychological debate to see to it that they have outgrown the crudity that fails to distinguish between questioning a thing's existence and describing what it is made of and how it is put together; between calling it a fiction and propounding the theory that it consists of a certain combination of elements, each simpler than itself, no one of which, taken alone, contains the essence or evinces the full character of the whole. One is put upon the old and obvious question: Where could Dr. Münsterberg and those who think with him fancy that we first obtained the notion of mental activity if not from some experience which the words could be held to describe?

The real situation, of course, is sufficiently plain. Upon the broad fact of the reality of mental activity or volition the disputants are agreed. Dr. Münsterberg maintains that it may be resolved into certain presentations—sensations or ideas derivative from sensation. Mr. Seth wishes to maintain that this analysis, while so far as it goes it may be correct, is inadequate to the facts of consciousness—that the most essential fact of all quite escapes it. What evidence is adduced to prove this—that the kernel of the will is not resolvable into presentations—it is at first difficult to say. He seems, as the citations above go to prove, to assume it as self-evident and to regard Dr. Münsterberg's obliquity as moral. But here, too, the real logic of the situation is not obscure. In effect he puts us, or ought to put us, upon our introspective powers and call on us to confess that the report of consciousness goes against the 'presentationists.' But since these latter form a not inconsiderable body of trained psychologists who read their consciousness otherwise (if we may be permitted to ignore the moral innuendo), the note of indignation ought surely to be subdued, or even hushed, leaving us to a calm comparison of the different results. And on this level Mr. Seth would indeed have one further remark to make; a suggestion to explain the 'presentationist's' failure to see what really is there. The ancient doctrine of the radical disparity of knowledge, feeling and will comes here to the foreground. Mr. Seth holds that what he calls the 'essence' of will is overlooked, because in a sense it does not *appear*; it 'cannot be held up as an object in the mental field of view'; like feeling, it is not a presentation or cognitive element; it can be experienced, but in the strictest sense of the words it can never be

known; so that from an introspective eye in quest of presentations only it naturally remains hidden.

This is far enough from the first time that a mental entity or agency charged with far-reaching powers and explanatory virtue has been alleged to be by its nature concealed from view. Indeed, 'essences' as well as 'occult causes' have in this a family likeness, that their potencies are apt to work themselves out in the dark. Nor has the fact that mind is a domain where reality is appearance been any bar to the introduction of a species of these secret powers and 'principles' within its pale. The exigencies that tempt to such suppositions are obvious. I cannot find, in volition for instance, any inward and 'spiritual activity' that can be distinguished from the anticipatory idea of the act and the sensations of effort; but such activity—so says metaphysical preconception—there must be; accordingly, it is there, but invisible. And it is not only in the non-cognitive realms of will and feeling that this seductively simple method has been followed. When Schopenhauer came to describe the *Begriff*, the same dilemma awaited him. Our true abstract conception of quality or class could not be of the nature of sensuous imagery. As to that imagery the nominalist was right; it could never consist of ought but fragments of concrete experience, definite or vague, and amongst those fragments we search in vain for the conception. But a true abstract conception there must evidently be; hence it was not of the character of 'corporeal imagination' (in Descartes's phrase), not a presentation (*Vorstellung*), not to be 'held up as an object in the mental field of view'; it was just the mind's essential unimaginable *grip* of the quality's or class's nature; there was nothing more to be said of it. Behold another invisible mental fact, this time not of the order of feeling or will, but in the purely cognitive realm. It was as unthinkable to Schopenhauer that the fragments of imagery that accompany abstract terms could be *all*—could by association call up the proper sequel and so send the mind on safely to the proper acts or further images—as it is to Mr. Seth that in will the anticipation and the sensation of effort are all—that their material counterparts are the causes of the act and they themselves the appointed mental precursors of a sense of the act's performance. Where the plight is the same and the preconception is the same, the suggestion of a kind of mental invisibility may be counted on to appear. The annals of psychology are rich in instances.

Meanwhile one submits that in the end it matters not a tittle whether it be said that the spiritual activity or whatever may be the central fact of will is 'known' or is merely experienced. 'Activity' seems par-

ticularly elusive for the reason that we are wont to identify it vaguely with physical activity or motion, which is not itself a concrete existence, but a relation—the abstract series of successive positions of a material existence. Physical activity is no more a concretion than *change* is a concretion. Let us remember that ‘mental activity’ must be something in this respect radically different. It is not a series of relations merely, it is a series of realities, of the psychic order. The perceptual image of a red brick house before us, whether illusion or no, is itself a veritable existence, and so is a toothache, a sensation of heat, an emotion of joy, a mental effort—any form of consciousness whatever. To be sure, Professor Ladd has been telling us (and a certain footnote shows that Mr. Seth is in sympathy with him) that these things are not existences, but acts of an existence; as thus: The image of a red brick house is the act of a unitary existence which we may call the self; but if we firmly repress any exclamations that may start to our lips at this combination of words, and patiently wait until Professor Ladd comes to explain in what the unitary existence consists, he has apparently nothing to fall back upon but its acts, that is, the images, etc., over again. A mental activity then can be nothing but a continuous succession or stream (for it appears to involve a lapse of time) of concrete mental stuff. Mr. Seth lays it down that in the case of volition we can never, in the narrow sense of the word, know this mental stuff. But if the stuff is capable of association (as he does not deny), and hence may have a name attached to it; if the name recalls to us the stuff (as Berkeley, whom he here invokes, expressly admits); and if of the stuff thus composing the word’s meaning we have a consciousness, an ‘awareness,’ an ‘immediate assurance,’ it is not plain how the case defies introspection on any reasonable use of that term. Our author writes: “We remember feeling only in the sense that, when the ideas which caused or accompanied it are recalled, they are recalled with the same tone of feeling; in other words, we re-experience in a fainter degree the feeling which we then felt,” and the same presumably applies to will. I do not consider this correct, but it is all we need here ask. Feeling and will may in this manner be recalled with the presentations to which they cling, and may be distinguished therefrom. Whatever, then, we say as to the senses in which ‘activity’ can or cannot be ‘held up as an object in the mental field of view’ (a fine specimen of the question-begging metaphor), at least it may be experienced, recalled, discriminated from things dissimilar, named and talked about. I do not know what more is requisite in order that it should be fundamentally on a footing in its manner of existence and appearance with the other concrete data of the mind.

Nor do I believe that Dr. Münsterberg or the other 'presentationists' (to keep to the ill-favored name that Dr. Ward has put upon them) have meant that will could be resolved into presentations or 'content' in any other sense than this. If they go on to say that these presentations are all sensations or copies of sensations, they mean no more than that they are, or in the first instance were, physically occasioned by sense-organ and nerve, like sight or sound. This does not prejudice their possession of an inward and personal quality peculiar to themselves or common to them with feeling only. It says nothing of their quality at all, but simply brings them into relation with certain outlying material facts. Therefore it does not touch some of the contentions upon which Mr. Seth chiefly insists.

The truth is that in effect Mr. Seth's tilt is against psychological analysis of the higher functions. The 'essence' proper shuns both analysis and observation—the former not at all because the 'essence' is indivisibly simple, for the properties attributed to it are highly complex. It is really because it is fancied that mystery is dignity and that to analyze is somehow to dishonor. The author extends his tilt even to physiology. We are misled, he says (pp. 110-11 et seq.), by words like currents, and energy, and least resistance, when we take them mechanically. "You cannot separate either the appreciation of the stimulus or the reaction upon it from the organism as a whole" and the 'life and purpose' that 'govern' it. "It is the living being as a unity that is aware of the sensation and responds to it." This is suggestively close to the theory of a vital principle, not yet extinct. The physiologist or the psychologist may carry on his analyses only if he signs a declaration at the outset that he means nothing by them; that the facts upon which he works are not to be analyzed. If our author had an acute interest even in the analysis of terms he would have discovered that the word 'unity' has a multitude of meanings, some of which are bound together by but a vague analogy; that in some senses of the word the mechanical theory of the nervous system acknowledges and reckons upon the unity of the organism as much as any other; and that, on the other hand, even if the atoms that compose an organism act according to laws fundamentally different from those of other atoms (which is anything but proven), they still presumably act according to some laws, and the computation of the influence of part upon part under those laws would involve analyses not inaptly to be called mechanical. The enemies of analysis can hardly have any objection of principle to that humble form of it which consists in the reflective scrutiny of what we mean by terms; and if they would

begin upon this inoffensive exercise their conception of the bolder forms could hardly remain unilluminated.

D. S. MILLER.

L'Évolution des idées générales. TH. RIBOT. Paris, Alcan. 1897.
Pp. 260.

Where other writers are content with a volume or two, M. Ribot is for a whole library in octavo. This latest of his many books, a résumé of lectures delivered at the Collège of France, is announced as the first of a series planned to embrace all the parts of psychology. Such industry is the more remarkable in that it results in literature which is always readable and worth the reading, for this reason at all events, that it to so large a degree affords a conspectus of contemporary opinion on the particular subjects treated. In this sort of work M. Ribot is an acknowledged master; he impresses one as a kind of professional auditor examining the psychological accounts.

The principal thesis of the present work is that abstraction and generalization progress by three great stages. There is (1) the stage of the 'lower' abstracts, prelinguistic, consisting in 'generic images.' Then there is (2) the stage of 'intermediate' abstracts in which images are accompanied, but not yet wholly replaced, by words. We can distinguish two classes, that in which the word is unessential and only in a slight degree an instrument of substitution, and that in which the word is indispensable and an instrument of substitution. The first class represents the 'concrete-abstract' stage of thinking, *e. g.*, counting among savages. Finally (3) there is the stage of the 'higher' abstracts where the only element in consciousness is the symbol, the word. The principal aim of the book is to trace out these three stages through their various subdivisions and transitions. This aim is, we think, in the main, successfully accomplished. There are one or two points, however, to which it is perhaps desirable to call attention.

In the first place, M. Ribot holds that some animals abstract, generalize and reason. We have no motive for disputing this; on the contrary, the indications seem to us strongly to favor this interpretation. But we should like to know more particularly concerning the process by which this is effected. M. Ribot refers us to 'generic images.' It is important, therefore, to understand what the generic image is and the method of its functioning. The most explicit statement as to its nature is on p. 101, where we are told that it is simple and of the practical order, that it results from repetition, that it is an extract of very obvious resemblances, and that it is condensed in a

sensible representation, the result of passive assimilation—in all of which particulars it is contrasted with the concept. Elsewhere we find the familiar comparison of it to a composite photograph. Nobody probably denies the existence of images formed in the manner suggested. But in what way are they generic? An image, whether distinct or blurred, definite or schematic, is always, as such, particular. It can only be called generic for one of two reasons, either because in its formation it is due to the action of a particular class of objects, or because it has itself generic functions. In either case, the important question here is, How does it come to have generic functions? How comes it about that the repetition of like experiences begets a capacity for conscious adaptation to new conditions, and what is the nature of the process in which such adaptation is actually made? M. Ribot has left this question obscure. He has cited a number of illustrations, chiefly from Romanes; what we would rather have seen would have been a careful analysis of one. The impression left at this point is of an attribution to the image, the conscious representation, of capacities far beyond probability and of a neglect of those unconscious and especially motor organizations which play so important a part in development later on.

But if the unconscious factors are neglected here they receive, perhaps, rather more than their due in the theory by which the author explains the 'higher' concepts. He finds, according to investigations already published and here resumed in full, that in a large number of cases when the subject is asked what he has in mind when he hears a certain word or phrase having general reference, the answer is, 'Nothing,' meaning nothing but what is heard, *i. e.*, nothing relevant. His theory is that what corresponds to this 'nothing' is an organization beneath consciousness of prior knowledge originally involving attention and effort, and that general ideas are essentially habits, intellectual habits. Mr. Stout criticises this theory (*Am. Psy.*, I., p. 83) on the ground that, while unconscious processes are doubtless involved, the state called understanding a word is a state of cognitive *consciousness*. The subject is too intricate to be pursued here, but this much must be added concerning M. Ribot, namely, that, while regarding the organization corresponding to the 'nothing' as unconscious, he attributes to it important effects, not only on the subsequent flow of thought, but on its present state. His theory is by no means to be identified with the 'habit'-theory of Hobbes. He strenuously opposes the position of the nominalists. The consciousness, apparently purely verbal, is sustained and vitalized, he says, by the potential knowledge, which gives to the

word 'not only its value, but its distinctive quality, like harmonics when added to the fundamental' (p. 150). But harmonics, though not separately audible, are still audible; it would seem, therefore, that the psychological element corresponding to the 'nothing' is not wholly unconscious.

One of the most interesting sections of the book is that in which the author, with ample use of anthropological material and of the results of modern psychological research, traces the history of certain 'higher' concepts in more detail. The concepts thus studied are number, space, time, cause, law and species. As a specimen we take the discussion of the notion of time. According to this, the abstract notion of time is derived from what for psychology is an ultimate datum, the consciousness of duration. This consciousness has its nucleus, probably, in rhythmic organic sensations. The elements which go to the constitution of the time-consciousness in general are probably motor and unconscious as well as intellectual and conscious. These positions are connected with a résumé of recent discussion and experiment. The first stage in the development of the notion depends on memory and imagination; it consists in thinking of a certain duration—a day, a week, a year—in a form more or less vaguely representable. This stage corresponds to the lower forms of abstraction, generic images and, at a later period, concrete-abstracts. At its lowest, it consists in generic images resulting from the repetition of like events, the beginning and end of each interval forming *points de repère*. The concrete-abstract form is seen among savages, who reckon the age of their children by the blossoming of the flowers, not for poetic reasons, but because they have not yet learned to separate the notion of time from the succession of events. The highest form of this stage of the notion is the popular conception of time as a vague entity rolling ever on bearing all events along with it. The second stage depends only on abstraction, and gives the pure concept of time in general, determined only by symbols. This stage was first reached, probably, by the early astronomers, and its general diffusion is due less to metaphysical theory than to the inventors of chronometric instruments. To the pure concept, time is infinite. M. Ribot, therefore, at this point discusses the conception of infinite time. His conclusion is that not only the notion is subjective, but time itself. If sentient life should disappear, he says, time would disappear with it, just as if eyes and ears were done away colors and sounds would go too. We all know the sequel to the argument; take away intellect and there is no longer an intelligible world, *ergo* the world is entirely relative to

and determined by the Ego. M. Ribot is thinking of no such consequence. He refuses to extend the argument even to the abstract idea of movement. After time has disappeared with the disappearance of consciousness, the revolutions of the planetary bodies, he says, would still go on as before. The objection, of course, to this is the impossibility of thinking it. We cannot think of movements in space—and the revolutions of planets are movements in space—as really happening except as successive and coexistent, *i. e.*, in time; if movements are there, a temporal order of events is there; no temporal order of events, then no movement. M. Ribot tells us that “consciousness is the necessary condition of any notion of time, which appears and disappears with it.” And so, we may add—for the statement read one way is a truism—of every notion. But if the relative clause applies, as the preceding argument shows it does, not to the ‘notion’ of time, but to time itself, that is a different matter—the question then becomes epistemological and would have to be settled, we think, on other grounds than those which M. Ribot has here brought forward.

At several points in the discussion, the question of the cognitive value of our general ideas is touched on. The question whether number expresses an essential feature in the nature of things, or whether it is merely a stratagem, a subjective device, is not decided (p. 164). The question as to the universal validity of the conception of cause is declared to be beyond the competence of the psychologist to settle (p. 212). The question as to the objective value of abstraction and generalization generally is declared to be one which psychology, as science of facts, can ignore (p. 219). Nevertheless, not only, as we have seen, does M. Ribot decide this question as regards the time-concept, but he decides in general that the position of the psychologist must be one of relativism, that for him our general ideas are approximations, having an objective value, but provisional and temporary, dependent on the variability of the phenomena and on the states of our consciousness (p. 220). This is argued on the ground that not only our ideas are in flux, but the order of the world and its laws also, and that nothing stable can result from two variable factors. Of course, if the principle be true, it must apply impartially, must apply, therefore, to the law of relativism itself. But waiving this, and admitting, for the sake of the argument, that it is true, it is clear that it only holds so far as the factors concerned are variable; it still remains to be considered whether there may not be some factor or factors on one side or both either absolutely or in some respect permanent. It is noteworthy that on the very last page of his book, M. Ribot refuses to deny to intelli-

gence an ultimate constitution underivable from experience. The fact is that this whole question is extra-psychological, and that the psychologist is as free to choose with regard to it as any one else. It is a mistake to say that his material forces him to adopt only the one side of the alternative.

In the last chapter M. Ribot discusses the question as to how the faculty of abstraction came to be constituted; also the different directions of its development. Abstraction he holds to be present in germ in the primitive sensations, perceptions, tendencies and emotions. The first condition of its development is attention. Attention is preparatory to dissociation. Then, by repetition of the dissociated elements, come the generic images, then the word is added, finally the word becomes pure symbol, dissociated from the images, now organized as intellectual habits. During and after this process, viewed historically, the development is determined by two principal causes, utility, which consists in simplification and is maintained by imitation, and the unexplained initiative of great inventors. Of the three directions taken by the development, the practical is chronologically first, the speculative next, the scientific last. The book concludes with a reiteration of what we take to be, on the whole, its most important thought, namely, that in symbolic thinking the substitute presupposes the actual existence of what it stands for, and that this is, in large part, if not, as M. Ribot appears to say, entirely, a mental organization below the threshold of consciousness.

H. N. GARDINER.

SMITH COLLEGE.

Outlines of Psychology. W. WUNDT. Translated by C. H. JUDD. Leipzig, Engelmann; New York, Stechert. 1897. Pp. xviii + 342. \$1.75.

In this work Professor Wundt has prepared an outline of his systematic views in psychology to serve as a syllabus supplementary to his lectures. In its English form—which seems to the present writer to be in every way adequate—it comes rather as an introduction to the author than as an available text-book; seeing that the necessarily condensed and at times decidedly dogmatic presentation, together with the lack of literary references or indications of different views even on the most debated points—seeing that these things are not calculated to arouse just attitudes of mind in the student. It appears also that the abstractness of method and the deliberate mode of analysis, whereby purely hypothetical ‘elements’ are treated, are directly calculated to

keep the subject in the bonds of a neo-scholasticism which, just in proportion as it prevails in our present-day instruction, hinders the advance of the movement toward the concrete and scientific treatment of the mental life. This is, of course, a matter of opinion; but when one remembers the courageous treatment which Professor Wundt always gives to his opponents, and the unhesitating frankness with which he himself states his views on such points, only equal frankness is proper in others who think his methods of analysis, together with his personal terminology, and his dogmatism on points of open debate, not in any way conducive to the advance of our science. At the same time, the full and experimental treatment of many problems of fact in the author's larger works will come at once to the minds of those who are familiar with them; and if this little book were read as an introduction, together with some other book written on a different plan, its unwholesome pedagogical influence would be in a measure counteracted.

Attention should be called to Dr. Judd's serviceable glossary of German terms at the end of the volume.

J. M. B.

Introduction to Philosophy: A Hand-book for Students of Psychology, Logic, Ethics, Æsthetics and General Philosophy.

By OSWALD KÜLPE, Professor of Philosophy and Æsthetics at the University of Würzburg. Translated from the German (1895) by W. B. PILLSBURY, Instructor in Psychology in the Cornell University, and E. B. TITCHENER, Sage Professor of Psychology in the Cornell University. London, Swan, Sonnenschein & Co., Ltd.; New York, The Macmillan Company. 1897. Pp. x + 256. \$1.60.

The original edition of this work in German has already received thorough treatment in the REVIEW, and the appearance of the English translation is simply a suitable opportunity for a special reference to its concept of the proper standpoint for psychology. Of the translation as such it is sufficient to say that, except in a few places, one is not conscious of its character as translation.

The first thing necessary to the working out of psychology, says Külpe, is a clear idea of the relation of physical and psychical—a delimitation of the two fields of natural science and psychology. In the history of the science this relation has passed through three stages, (1) where the psychical was identified with the vital principle, (2) where psychical and physical were the objects respectively of internal and external perception, and (3) where the psychical is coextensive

with 'subjective,' *i. e.*, with what is dependent upon the experiencing individual, and the physical is 'objective,' or that which is independent of the subject. The last concept has been elaborated theoretically by Mach and Avenarius but Külpe claims to have been the first to give an exposition of psychology from this standpoint.¹ It is based upon the fact that experience is unitary. The chair, for the naïve consciousness, is not two experiences, one of the object and another of a copy, but one experience which may be viewed equally as a sensation and an object. For scientific method the object is likewise the presented experience with the subjective accretions stripped off. The subjective and objective worlds are not, therefore, the objects respectively of separate internal and external senses but abstracted aspects of the unitary world of experience. 'Sensation,' 'idea,' 'perception' are names for subjective factors; 'objects,' 'properties,' 'states' and 'relations of objects' are expressions for the objective factors. "We may call the relation between the subjectified constituents an *association* and that which holds between the objectified processes a *mechanical* connection."² Of course, there are some facts of experience, such as memory images and feelings, which are purely subjective.

After this statement of the standpoint of psychology one is not unprepared for Külpe's argument for the *possibility* of a mind-substance, though he is careful to state his position in regard to its actuality as purely non-committal. But one may wonder why mind-substance has less claim to reality than material substance. For matter and mind are, according to Külpe, equally abstractions from the whole of experience. Each, in so far as it appears in experience, falls short of an ideal completeness. If one abstraction requires an idea of substance, why not the other? The difficulty seems to lie in Külpe's attempt to identify his own standpoint with that of Wundt, whom he classes³ among the advocates of the new psychological standpoint. For to Wundt,⁴ experience as such is not wholly indifferent. His 'subjective' and 'objective' are the data respectively of immediate and mediate experience; and it is the immediate character of the 'subjective' which renders the concept of a mind-substance meaningless. By this description of 'subjective' as 'immediate' Wundt makes it clear, notwithstanding his definition of the latter term, that he has never

¹ Grundriss der Psychologie, 1893, tr. 1895.

² p. 206.

³ p. 204.

⁴ Grundriss der Psychologie, pp. 3, 367, 368.

really departed from the concept of psychology as the science of inner experience and of experience *per se* as somewhat more psychical than physical. His conception of psychical and physical is, therefore, not at all that of Külpe, and the latter, in attempting to identify the two, has made his otherwise clear statement of his position somewhat equivocal.

WARNER FITE.

UNIVERSITY OF CHICAGO.

The Evolution of the Idea of God: An Inquiry into the Origins of Religion. GRANT ALLEN. New York, Henry Holt & Co. 1897.

Mr. Allen's attempt to reduce religion to a cult of the dead, and to derive the idea of God from the worship of a venerated or dreaded ancestor is far from satisfactory. There is throughout this laborious inquiry the atmosphere of special pleading; all manner of facts are whipped into line in order to certify a particular theory which has evidently controlled his method of investigation and predetermined its results. The main thesis of this work is that the customs of disposing of the dead among primitive peoples indicate the various stages of belief in reference to the state and condition of the departed, and consequent tendencies to revere and worship them. The most primitive custom is the preservation of the corpse on high platforms in huts or tents, resulting in mummification. An advanced stage is that of burial of the dead, and a still higher is reached in cremation. Corresponding to these three stages the following ideas in reference to the dead are entertained: In the first, they are thought of as bodily living; in the second, death is regarded as only temporary, a resurrection of the body being confidently expected; in the third, the soul is regarded as distinct from the body, and maintaining a separate existence after the body is wholly destroyed. In the highest stage, the spirit of a brave and great chief, admired or feared in his lifetime, comes to be worshipped after death. This is the beginning of ancestor worship, out of which polytheism gradually arose as the process of deification of chiefs or kings developed. Mr. Allen finds also that the worship of stones, stakes, trees, totems, sacred wells, etc., all came from early association of these objects with the burial of the dead. The sacred stone was originally a headstone of the grave, or the pile of stones thrown upon the grave to imprison more effectually the interred body. He gives similar explanations of the other objects of worship. The priest was an attendant at the grave of the dead; sacrifice was an

offering to appease the dreaded spirit of the dead, etc. Polytheism being therefore a sublimated worship of the dead, the rise of monotheism is explained as the unique experience of the Hebrew race who came to worship exclusively one of the Semitic deities, Jahweh, originally an ancestral sacred stone. As regards the founder of Christianity, he traces his origin to that of the corn-god of which Jesus is the survival. Mr. Allen's argument may be criticised at many points. I have chosen what seems to me to be the most crucial for brief mention :

1. His three forms of burial do not correspond to the three parallel views concerning the dead, and consequent forms of worship. All three stages exist among Australian savages, with corresponding distinction in reference to belief or worship, as is cited by Mr. Andrew Lang in the *Contemporary Review* for December. In many cases also of body-preservation and body-burial, there is a distinction in the savage mind between the body and the spirit. In reference to the burial customs of the Peruvians and their beliefs concerning a future life, Dr. Réville¹ says: "There was not the least idea of a resurrection of the body. If the corpse was preserved, especially in the case of departed Incas, it was because the Peruvians believed that the soul which had left it, still retained a marked predilection for its ancient abode and liked to return to it from time to time; and also, because they attributed magic virtues to the remains thus preserved."

2. Mr. Allen ignores the prevalence of Animism and nature-worship among primitive peoples. The worship of the dead was not the exclusive form of worship, nor was it more widely practiced than the forms of Animism, or of nature-worship. Mr. Allen insists that, when once the idea of the sacredness of stones associated with burial got firmly fixed in the savage mind, then a spirit would be regarded as also indwelling in other stones resembling the sacred stones of burial. This is pure conjecture on Mr. Allen's part, and even if admitted will not account for the objects of nature generally in which spirits were supposed to dwell and which could have had no possible association with the dead. Nature-worship, and that on a very large scale, disproves Mr. Allen's fundamental propositions, and yet his attention is so concentrated on the worship of the dead that all other forms of worship are not noticed by him, or else purposely ignored.

In Dr. Réville's examination of the early religions of Central America, Mexico and Peru, he is led to this conclusion, which is wholly at variance with Mr. Allen's position, namely, "that it is the phenomena of nature, regarded as animate and conscious, that make

¹ Hibbert Lectures, 1884, Albert Réville, p. 236.

and stimulate the religious sentiment and become the objects and adoration of man."¹

3. Mr. Allen mistakes the Hebrew religion as a unique instance of monotheism. There were intimations of monotheism among the Greek philosophers, the profound conception of one God as in Plato, and the τὸ θεῖον, the divine, of Anaximander. As Windelband remarks: "The transformation which the Greek myths had undergone, as well as the import given them in cosmogonic fancy as in that given in their ethical interpretation, tended everywhere toward a monotheistic culmination."² Dr. Réville also instances African tribes having a monotheistic belief. Indeed, Mr. Grant Allen has not dealt with the question in a scientific spirit. The difficulties attending the collection of facts among savages, and the obscurities incident to their interpretation, should not be further complicated by a restricted point of view and a biased judgment. The beginnings of religion must always be enveloped in the darkness of immemorial ages, and a mere conjecture cannot be maintained with dogmatic insistence, especially in the case of accredited facts which make against it.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

RECENT BIOLOGY (I.).

1. *Année biologique: Comptes rendus annuels des travaux de biologie générale.* YVES DELAGE. Première année, 1895. Paris, Reinwald. 1897. Pp. xlv+732.
2. *Essays.* G. J. ROMANES. Edited by C. LL. MORGAN. London and New York, Longmans, Green & Co. 1897. Pp. 253.
3. *Darwin and After Darwin, III. Isolation and Physiological Selection.* G. J. ROMANES. Edited by C. LL. MORGAN. Chicago, Open Court Pub. Co. 1897. Pp. viii+178.

In this handsome volume (1) Professor Delage begins the annual issue of a summary of biological progress; a work which was well begun in the earlier volume on 'Heredity, etc.,' noticed by Dr. Davenport in the November, 1897, issue of this REVIEW. In the Preface to this volume we read: "To those who have read our volume on 'Heredity and the Great Problems of Biology,' this new annual will not cause surprise. It is the natural sequel to that work. * * * The earlier work may be considered as a first volume, serving the purpose

¹ 'Hibbert Lectures,' 1884, Albert Réville, p. 38.

² 'A History of Philosophy,' Windelband, p. 34.

of setting the questions, defining the problems, tracing the outlines, establishing the categories, and resuming the results, up to 1895, from which date this periodical takes up all the topics and carries them on from year to year." The reader of the notice of the 'Grands Problèmes'—or of that work itself—will have, therefore, a fair idea of the divisions, headings, etc., of this new publication. The way in which the general purpose of the editor and his contributors is carried out in this first volume calls for much admiration. Not only will it be of great value to biologists; but students in neighboring departments, especially in psychology, will find it a reliable and readable introduction to the newer biological problems in their latest phases. One feature strikes the present writer as peculiarly good—albeit exceedingly difficult—*i. e.*, the attempt of the editor to gather up in a few pages a statement of the advance made during the year under each great heading, thus giving a résumé of each of the successive résumés of literature made by the contributors. Such a 'skimming off of the cream' could only be done by a master, and must in any case involve some personal equation; but Professor Delage has shown in his earlier work the sort of grasp on the entire subject—both as to information and as to judgment—which such an undertaking demands.

The allowance of space to psychology, under the head of 'mental functions,' is adequate and just. It is sincerely to be hoped that the editor will not take the advice of certain reviewers and restrict this department in future issues. Not only is this section of value to psychologists, as bringing their work into organic connection with biological results, but even more to biologists, who are thus informed of the light which psychology, especially in its genetic and evolutionary phases, is coming to throw on some of the standing problems of biology. This is seen in the volume before us in the statement of recent advances in the questions of instinct, individual adaptation and determinate evolution.

The department of mental functions in its more strictly psychological aspects is in the competent hands of our valued collaborator, M. Binet, who writes a very interesting section summing up the state of opinion in many experimental topics.¹ Several long analyses are contributed by M. Marillier (among them analyses of Sergi's 'Dolore e Piacere' and Baldwin's 'Mental Development in the Child and the Race'); M. Szczawinska contributes an article on the 'Structure of the Nervous System,' and we find Professor Ll. Morgan's paper on 'Some Definitions of Instinct' reproduced entire.

¹ The section on memory has been reproduced by Professor Warren in the *Amer. Naturalist*, October, 1897.

On the whole, therefore, we may count this publication as a distinct addition to the apparatus of the natural sciences, and extend congratulations to its learned editor and his collaborators.

The two posthumous works of Romanes (2 and 3) are valuable additions to our legacy from that acute mind. The book of essays is of less importance than the other, seeing that it is a collection of papers published at various dates, which do not in all cases represent the latest and most matured opinions of the author. They all have biographical value, however—meaning mental biography, of course—especially those of a more practical character, which bring out human points of view. In the other work, we have the systematic exposition of the theory of Physiological Selection, which is possibly Romanes' most original and interesting single contribution to natural history.

This theory has two main features; features which should be taken separately, I think, and which only lose in force and serve to introduce confusion when brought under a single point of view, as Romanes does. The real novelty of physiological selection consists in the hypothesis that congenital variations toward infertility might lead to relative segregation in a group of animals living together, with the development of the groups thus segregated from one another in divergent lines. No one who appreciates the problem of inter-specific infertility can, I think, fail to see the force of this hypothesis, nor fail to agree with Romanes—quite apart from the evidence of fact—in the hypothesis that specific differences may be secondary to sexual variations, rather than the reverse, as Darwin supposed. I cannot help thinking, however, that Romanes places too much confidence in the so-called 'principle' of Weismann (*amixia*) and Delboeuf, that any slight average difference between different groups must develop itself. That would seem to depend upon circumstances; and at any rate it is purely hypothetical. Romanes weakens his case by making it a sort of corner stone to his structure; for whatever the causes be of the subsequent divergent evolution—say Wallace's pure utility view—the original segregation by physiological selection would lose none of its value, if it be true; especially in cases of absolute infertility. The value of physiological selection as producing divergent species would seem to rest in cases of relative or partial infertility largely upon the sort of variations which were correlated¹ with the infertility—a point which the theory of Reproductive Selection of Professor Karl Pearson covers. His theory

¹As to whether partial infertility, alone, without any regular correlations would produce divergent results seems very doubtful except as it tended to result (by accumulation of variations) in absolute infertility. This latter result Romanes himself supposes.

is spoken of further below (II.) and its difference from Romanes' pointed out. What they have in common is the postulate of infertility, Romanes assuming its segregation value and so finding it available to produce divergent or what he calls 'polytypic' evolution.

The other point of which Romanes makes so much—and, I think, unfortunately—is that in which he agrees with the Rev. Mr. Gulick, the writer who first proposed and has elaborately expounded—but under different terms—the principle of physiological selection. Both of these authors, Romanes later so far as one can gather, formulated the general principle of 'Isolation'; meaning by it—to gather the matter up briefly—any sort of relative control of pairing. If, for any reason, males *A* to *Z* can pair with females *a* to *z*, but can not pair with females *m* to *n*, the males are 'isolated' from the latter females. Under this 'principle,' the author shows, everything 'in heaven above and on earth beneath' can be brought. Natural selection is only a case of isolation, so is the migration of Wagner, and the geographical separation of Weismann, and physiological selection from infertility, and artificial, and indeed sexual selection. He says: "Equalled only in its importance by the two basal principles of heredity and variation, this principle constitutes the third pillar of a tripod on which is reared the whole superstructure of organic evolution" (p. 2). With all the labored proof of this proposition, it suffices to say that it is true, because self-evident; and at the same time, in the present state of biological science, well-nigh worthless. For the very concept of heredity through sexual reproduction presupposes it. All heredity in *particular* involves the 'isolation' of the two parents temporarily for the purposes of the act of mating. We might even go so far as to announce a great 'principle of negative isolation' (!), *i. e.*, that by artificial selection, or any sort of human regulation, the upper limit to the birth-rate in any species may be set by the isolation of the male from more than one wife. But surely it adds nothing to natural selection to call it isolation, explaining that it depends upon the elimination of some individuals and the consequent isolation of those not banished to the shades; nor does it add anything to the other sorts of selection now historic, both as facts and as having names, to call them 'isolation.' All this seems to the present writer to furnish evidence of the tendency of Romanes, shown also strikingly in his later writing on the inheritance of acquired characters, to lay too much value on logical disquisition.¹

¹ As to the minor utility of showing that there is such a wider though negative category under which certain of these natural processes may be viewed—that no one, I suppose, would dispute; but when it comes to considering it a great dis-

In thus dwelling on the striking features of physiological selection, as Romanes and Gulick have developed it, I by no means mean to lead the reader to think that this important theory is done justice to; on the contrary, the book will be found, from many points of view, to build up a claim for this hypothesis as representing a real factor in evolution—especially in divergent evolution—which writers who refuse to recognize it, as Mr. Wallace, will have great difficulty in disposing of. And this the more when it is taken in connection with the evidence which Professor Pearson gives to show that ‘Reproductive Selection’ (on the basis of relative infertility) is actually at work.

For example, among a certain class in a community, a high relative death rate among women of narrow hips may serve to establish a correlation between maximum effective fertility (in Pearson’s sense) and broad hips; while in another class in the same community the same maximum fertility may perhaps be established by intentional regularity of size of family with better medical attendance, without any reference to size of hips at all. Here there would be a tendency to divergent evolution in the matter of hip conformation, due simply to ‘isolation’ by a social barrier. Romanes’ hypothesis calls for the same result where the barrier is the physical one of some degree of gross infertility between the two groups. I put forward this social instance because, among other reasons, while it is one of the few forms of ‘isolation’ which were not already recognized and named, yet it is one of the forms which these writers did not recognize nor name. It is also interesting as showing a type of cases in which groups *living together* (that is, not geographically separated), and at first quite fertile *inter se*, might acquire infertility, as a consequence of other morphological changes, thus illustrating Darwin’s view, but under Romanes’ conditions. I have called this choosing a mate under social limitations ‘personal selection,’¹ but, like all the other ‘selections,’ it can be scheduled under ‘isolation,’ of course.

It is interesting, also, to note that Darwin recognized several forms of isolation (see Romanes’ quotation, p. 108, note) besides geographical separation; and among them two forms which involve physiological selection, *i. e.*, ‘breeding at slightly different seasons,’ and covery, and requiring biologists to adopt a new terminology with a view to recognizing it, it would seem to be going too far. Nor does this suggest any disparagement of the fresh and new considerations advanced, especially, in Mr. Gulick’s very notable papers. A similar classification of certain of the special ‘factors’ under the general head of ‘isolation’ is made by F. W. Hutton in *Natural Science*, October, 1897.

¹ Baldwin, *Social and Ethical Interpretations*, Sect. 40 and Appen. B.

'individuals preferring to pair together' (sexual selection). The latter is a case of physiological selection, if only we make the highly probable assumption that the 'mental preference' for certain mates carries with it maximum fertility with those mates.¹

J. MARK BALDWIN.

(To be concluded.)

CHILD-STUDY.

- I. *Notes on Children's Drawings*. Ed. by E. H. BROWN. University of California Studies, Vol. II., No. 1, 1897. Pp. 75.
- II. *A Study of Children's Drawings*. H. T. LUKENS. Ped. Sem., IV., October, 1896. Pp. 79.
- III. *Remarks on Tickling and Laughing*. H. M. STANLEY. Amer. Jour. of Psychol., January, 1898.

Two of these papers (I. and II.) are interesting contributions to the facts of the early stages of the child's learning to draw. Professor Lukens's appeared first. It contains examples of early drawings, as does also the other. Both compare their results and the drawings with those already published by Baldwin and Sully. Fuller literary citations will be found in the paper by Lukens. The California Notes are by different persons, one set being from the practiced hand of Miss Shinn. It is to be hoped that Professor Brown will continue to collect and edit such good work.

The paper of Professor Lukens is better than most of the matter published in the *Pedagogical Seminary*, and in so far justifies the eulogistic description of it, made with the customary praise of its own

¹This is a correlation which I have never seen suggested anywhere; yet if it should be true, Mr. Wallace would have to admit physiological selection as a sort of organic counterpart of his selective association by recognition marks. Without such a correlation, sexual preference would seem to lose much of its biological significance. I speak further below of such a possible correlation in remarking on Professor Pearson's social conclusions. It might get some support from the fact that the coyness of the female, which, on the hypothesis of Gross ('*Spiele der Thiere*;' suggested earlier by Guyau, '*Non-Religion of the Future*,' p. 302), plays an essential part in sexual selection, demands increased strength and persistence in the male's impulses. It might be made a matter of experiment to determine whether highly-colored, grand-mannered birds are either absolutely or relatively very fertile; or it might be observed whether sexual-criminals (in whom the impulse on the mental side may be considered strong) have unusually large families, or progeny later in life than others—both, however, very complex problems involving other factors.

publications, which now comes out regularly in this journal. Undoubtedly in so doing the editor sets a candid example to others to say what they think also. And a few remarks on the sort of results in so-called 'child-study' emanating from certain quarters may not be amiss, taking as a text the very strong points made, in the same direction, by Stanley in his paper (III.), which everybody should read.¹

In the opinion of the present writer, results obtained by the syllabus method have very little value. They lack the first requisites of exact method; and moreover they are often further vitiated by a certain speculative philistinism and crudity of result. The syllabus method is bad through and through. There is absolutely no way to control the reports. The present writer issued one syllabus (on the 'Social Sense'), adding to it a formidable set of conditions to be strictly observed (for thus only could the results have value). The returns which it brought were filled with apologies for not having carried out the conditions!—and the results have little value when tested by the plainest rules of scientific control. Now, what can be said of indiscriminate observation of every conceivable thing which the children do, when the uninstructed teacher or parent, worked up to a fine pitch of enthusiasm for his new scientific calling, notes and describes, 'with no eye to pity him and no arm to save'? This should be said in direct antagonism to the movement which is now laving the shores of both oceans. Child-study is a fad, a harmless one for the most part;² indeed, a beneficial one to those teachers who lacked humanity before and are now finding it in their attitude toward their pupils. But it is an insult to the teaching profession to tell them that their humanity needs this sort of cultivation, and to hoodwink them into thinking that they are making contributions to science. As to the positive outcome of the child-study 'movement,' so-called, it will be found in summary at the end of Miss Wiltse's second article in the *Ped. Sem.*, October, 1896, 124-5. One may see at a glance how much the expenditure of money and time on lectures, conventions, printing, etc., is bringing in the way of returns, as stated even by an enthusiast. I think the time has come for the emphatic expression of opinions in this matter, and I only add one voice—raised, indeed, some time ago—to the expressions made strongly already by Münsterburg, Butler and Bryan.³

¹ Stanley's paper is an examination of the earlier one by Hall and Allin on the topic of his title (which this notice does not take up).

² Professor Münsterberg thinks it is not so harmless. See the *Atlantic Monthly*, February, 1898.

³ See also the very discriminating article by Bailey in the *North-Western*

About the other tendency spoken of, possibly the least said the better. Yet, when theory is being attacked for being open and conscious of itself, it is well to point out the sort of crude theory which lurks behind the very denunciations of empiristic philistinism. Many of the papers in the *Pedagogical Seminary* contain hints of valuable points of view, which, if worked out, might bring some order into the dreary mass of facts, reports and syllabus returns; but no, facts must speak for themselves; we may put them in tables, and if a curve, a holy curve, can be drawn, that is rich, though it bar the way to thought. Let no man think beyond the confines of the curve. If a general principle be enunciated let it be with that positive conviction and assurance of demonstration which curve- and table-evidence avail to produce. No hesitation here: "You know or you don't know, and that's the end of it!"

Can curves apply themselves? Can tables digest themselves? Can the teachers be benefited by purchasing tables and curves and scanning them with the fine frenzy into which the more inexperienced of them are being worked? To be concrete, what is the scientific value of the Worcester collection of observations on imitation? Value they may have, of course, if confirmed and interpreted; but who can interpret such facts except by resorting to general hypotheses which are openly and avowedly theoretical? And further, I submit that no results which are not gathered avowedly under the critical eye of interpretation can ever afterwards be available at all. Professor Sully's book seems to me to illustrate the way a man may be led astray by the use of just such results as those of which I speak.

J. M. B.

Memory and its Cultivation. By F. W. EDRIDGE-GREEN, M.D., F.R.C.S. New York, D. Appleton & Co. 1897. Pp. 311.

This work, which forms one of the recent volumes of the International Scientific Series, deals with the subject of memory both from a scientific and from a practical point of view. Although more than two-thirds of the book are given to the theoretical exposition in the first part, yet the treatment throughout seems to be determined with reference to its bearing on the problem of the training of memory.

On the basis of the distinction between sensory and motor nerves, the author divides the phenomena of memory into two great groups,

Monthly, January, 1898. Stanley says in the paper cited: "As to the method of investigation, the questionnaire mode of popular reports is evidently a crude and very tentative form."

sensory and motor; the sensory group including "the memory of all ideas, emotions and other processes of the mind arising out of the sensory memory impressions." Under the two great groups come the special memories which are developed in each case owing to special intensification of certain portions of the general memory. Both sensory and motor memories are constituted by impressions, *i. e.*, modifications of the protoplasm of the cells in the memory centres, and associations between the cells so modified. The memory of internal impressions differs only in this, that the memory is modified not by external impressions, but by internal impressions originated in the higher parts of the brain. "The mind is made up of a number of faculties, each of which responds to certain impressions, and influences the mind as a whole to seek after those impressions and to avoid their negatives." There are perceptive faculties and intellectual faculties; the former respond to external stimuli, while the latter have to 'convey certain impressions from the sensory memory to the mind.' They agree, however, in this, that "for each faculty and for each individual, a psycho-physical series can be constructed and, according to the units of this series, ideas of resemblance or dissimilarity between impressions will be obtained." A large faculty invariably implies a powerful memory; it is the function of each faculty to perceive the impressions of which the memory is made up. We require then to know what the faculties are. Of classifications of faculties, that given by the phrenologists is "certainly the best system extant, as far as the discovery and definition of ultimate faculties is concerned." Modifying this system in a few particulars the author next proceeds to state and define 37 ultimate faculties, which include faculties of sensory perception, of composite perception, reason and imagination, of motor organization, and lastly of propensities and emotions. Special prominence is given to the author's theory of color perception. The idea underlying this extended treatment of faculties is that the reader may be enabled to observe himself and note the relative strength of the different tendencies and abilities in his character, and, on the basis of this, learn how he may most easily, by the use of one or other, secure a distinct and intense impression of some given subject matter.

Dr. Edridge-Green argues strongly against the theory that the cerebral seat of memory is the same as that of perception. One line of proof is found in pathological cases where there is loss of a definite kind of memory, but no corresponding loss of power in the corresponding faculty. Another objection to the theory rests on the contention that if it were true then each particular faculty would remember

separately its own impressions; thus in a complex visual perception we should have separate memories for color, form, size and comparison, since there are separate faculties for each of these phenomena. To the author's mind it is much more simple and reasonable to suppose sensory and motor memories stored up together, each group in its own centre. He considers that the thalami optici are appropriate centres for sensory memory, while the corpora striata subserve the motor memory. Positive evidence in favor of this hypothesis is given in the chapter on localization of memory. The centre for sensory memory must have a relatively independent physiological existence; the thalami optici fulfil this requisite, in addition to supplying a basis for the distinction of perception and memory. It is true that in a report on 50 cases of disease of basal ganglia which is referred to, no defect of memory was reported. But then, Dr. Edridge-Green notes, in no case were both thalami optici diseased and the defect of memory might have been overlooked.

It will be seen from the foregoing summary that the account of memory presented in this volume is undertaken from a physiological standpoint. There is no clear conception of the relation of the impressions to the mind which perceives them by means of the various faculties of which it is made up; the impressions are conveyed to the mind by faculties which are now called faculties of mind and now faculties of brain. It is no more satisfactory to find a scheme of faculties presented as a scientific explanation. This is not merely out of place as a psychological statement; it vitiates also the author's reasoning in regard to the localization of memory, in leading him to suppose that the theory which he opposes logically implies separated memories corresponding to separate faculties. A locally separate organ of memory does not seem specially to facilitate the physiological explanation of retention and of the thoroughgoing connection of presentative and representative elements in mental life.

The most important chapter in the first part is that dealing with remembrance, recognition and recollection. Three laws are given for remembrance—involuntary revival—which may be briefly stated as follows: (1) impressions tend to revive those of a similar character; (2) revival of one impression by another leaves the two separate unless the revival is accompanied by conscious recognition of similarity; (3) revival of one component of an impression tends to the revival of the remaining components. In regard to the first, careful attention is given to the conditions by which the intensity of the reviving or revived impressions and the consequent memory are determined. The second

law is used to explain cases where mere unthinking repetition of an impression leaves no clear and exact memory. Recognition, the conscious association of impressions according to the second law, may be either direct or labored. Recollection is a voluntary process of recall, stimulated not by perception but by idea; it also presents two forms, instantaneous and labored. There are many valuable observations in this chapter, but the treatment would have been more clear and thorough if the whole explanation had been brought more into line with processes ordinarily recognized in the psychology of association.

Chapters on the connection of motor and sensory memories, on memory in the lower animals, and on variations in memory, normal and pathological, occupy 61 pages of the book. Nowhere does the author give a hint of the work done by Ebbinghaus and those who have followed in the path opened up by him. Such an omission has no justification.

In the second part on the cultivation of memory, Dr. Edridge-Green gives a number of practical rules for the improvement of memory. In the preface he states that they have enabled him to learn a subject in about a fifth of the time formerly taken. A few of the rules may be briefly indicated. Let the first impression be as intense and distinct as possible, but not too detailed. Revise what has been learned as frequently as possible and without reference to the original source. Bring an impression into connection with former impressions where possible; when a new subject is being taken up form relations with something similar in former experience and connect all the members in a series closely together. Use by preference the largest faculty, use as many faculties as possible, and employ each faculty when the mind is fresh and the nervous force is abundant. Interesting suggestions are also made as to various systems by which dates, events, etc., can be committed to memory by means of words and phrases. The rules and suggestions here given may be of great service to many who have not reflected on the best means for utilizing the memory power which every one possesses in varying form and degree.

W. G. SMITH.

SMITH COLLEGE.

VISION.

In Sachen der Optischen Tiefenlokalisation. HILLEBRAND. *Zeitsch. f. Psych. u. Phys. der Sinnesorg.* Bd. XVI., pp. 71-151.

Hillebrand is a supporter of Hering's theory of space-perception. In Vol. VII. of the *Zeitschrift* he reported an elaborate series of ex-

periments on accommodation, on the basis of which he criticised the earlier experiments of Wundt, rejected that writer's conclusion, and offered arguments in defense of Hering's theory. The experiments of Hillebrand have been repeated and enlarged by Dixon in England and by Arrer in the Leipsic laboratory, and his conclusions have not been acceptable to these investigators. The present paper by Hillebrand reports no new experiments, but aims to review the whole controversy and to defend his original position. In fact, there was no necessity of new experiments, since there is general agreement as to the facts involved. The disagreements are disagreements as to the value of various methods of experimentation and as to the true interpretation of results.

For the sake of clearness it may be well to depart somewhat from the order of Hillebrand's discussion as well as to recall some of the facts referred to only indirectly in the present paper. The essential facts on which the discussions depend are as follows. Given as the only object a mathematical line (this can be produced as the boundary between two surfaces), if this line is viewed monocularly and then is gradually moved back and forth, it will be found that its different depths are not perceived. If in a second case such a line be fixated monocularly and then be suddenly replaced by another similar line at a different distance, the change will be perceived, if the difference in distance is great enough. If instead of such a mathematical line an object be used the dimensions of which are so small that no perceptible changes can be caused by varying its distance, as a thread for example, and the distance of this thread be compared either successively or simultaneously with the distance of a second thread, it will be found that here, too, if the difference in distances is great enough, the difference will be perceived. Finally, if such a thread is viewed binocularly and compared to a second thread shown subsequently, it will be found that the distances at which changes in depth can be perceived are about the same as in the monocular experiments. The first experiments with mathematical lines give absolutely negative results; the others are positive, but the distances are so great that even these positive results call for explanation.

The facts, as already remarked, are in general agreed upon by all the investigators; the interpretation is difficult because it is not certain what conditions are present in each of the cases described. Hillebrand regards the first series of experiments as conclusive, and holds that they show that accommodation does not furnish the data for the perception of depth. He finds in each of the other cases a factor other than

muscle-sensations, which explains the positive results. Thus in the cases of sudden change there are an indistinctness of the image and a voluntary change of accommodation in the effort to secure clear vision. The direction of the accommodation is known and it may be used as the datum for inference, but is not the source of the perception. In the cases where objects are used, the changes in size of the retinal images are the important factors. Arrer's calculations, on the ground of which he asserts that the changes are too small to be perceived, do not agree with the introspective evidence given by his subjects and make the mistake of regarding the absolute threshold and difference threshold as the same. Binocular perception of depth is due to the differences of the two retinal images. Even the localization of the '*Kernpunkt*' is due to such differences, for the double images of parts of the body and of intervening objects form a regular series of differences from the body to the *Kernpunkt*. Thus all judgment of depth is relative to the localization of the body itself.

Arrer takes an entirely different attitude towards the results. For him the positive results show conclusively that accommodation does furnish data for the perception of depth. The negative results are the ones that he must explain away. This he seeks to do by showing that the mathematical line never had any definite localization and therefore there could be no judgment of change from any point. Furthermore, the line is vague on account of irradiation, and its movements are too gradual to allow perception of change. The last two objections Hillebrand regards as trivial. The light and movement should be properly adjusted. The first objection begs the very question under discussion. The absence of all absolute localization is the foundation of all his conclusions.

This whole discussion is a good illustration of the difficulties which confront experimental psychology. The facts are not in dispute, but the conditions under which the facts were obtained, are. The fundamental difficulty is that the theories of space-perception with which the investigators began their experiments and in the light of which they interpreted their results, can both assimilate the facts without inconsistency, so long as the conditions are not fully understood.

A further criticism against all monocular experiments is that the influence of the closed eye is by no means eliminated. This fact is recognized by all these investigators. Hillebrand even attempts to extend his negative conclusions so as to cover not merely accommodation, but conveyance also. Evidently it is not shown that the closed eye may not, by some irregularities in its movement, tend to complicate

the muscle-sensations in such a way as to be a positive disturbing factor. The influence of the closed eye must be taken into account.

The whole question is open and must remain so until facts enough are discovered to make it clear what conditions are really involved and which interpretation is true to the facts, not merely capable of covering some of the facts.

CHAS. H. JUDD.

WESLEYAN UNIVERSITY.

Die verschobene Schachbrettfigur. HUGO MÜNSTERBERG. *Zeitsch. f. Psych. u. Physiol. d. Sinnesorg.*, XV., 184. 1897.

In his article on the illusions of Zöllner and Loeb in Vol. XIV. of the *Zeitsch. f. Psych. u. Physiol. d. Sinnesorg.* (noticed in the January number of this REVIEW, p. 94), Heymans brings forward an optical illusion which he reports having found in the Milton Bradley collection entitled 'Pseudoptics.' The illusion consists in the apparent deflection of a straight line, on either side of which are arranged several pairs of squares in such wise that the inner edges of each pair overlap by half the length of the edge. The brief comments accompanying the collection state that the illusion is due to *irradiation*. This explanation Heymans briefly discusses and unconditionally rejects. Until quite recently the compiler and arranger of this valuable publication known as 'Pseudoptics' had remained unknown. But in the article before us Professor Münsterberg assumes entire responsibility, and states that the illusion in question was accidentally discovered by him upon a railway season-ticket, the squares of which were alternately light and dark, and arranged as if in the form of a checker-board, each row of whose squares has been pushed along by its neighbor to the amount of one-half the edge of each square. The author mentions that all explanations given in the collection are intended for the general public and must therefore often appear incomplete and unsatisfactory to the specialist. He is convinced, however, that irradiation is the explanatory principle to be made use of in the present case. Still, it is not denied that the illusion may, in part, be described in the language of other theories.

This is not the place to discuss the issue between Heymans and Münsterberg, but the writer of this notice hopes in the near future to report a somewhat detailed examination of this illusion.

A. H. PIERCE.

AMHERST COLLEGE.

ETHICAL AND COMPARATIVE PSYCHOLOGY.

Psychology of the Moral Self. By B. BOSANQUET. London, Macmillan & Co.; New York, The Macmillan Co. 1897. Pp. viii + 132.

The author thinks the time has come for some *rapprochement* between ethics and the new psychology. Although "psychology is still full of controversy, and fundamental questions are *sub judice*," yet "the doctrine of Apperception, and such an idea as that of 'vital series,' * * * are far enough advanced to throw a wholly new light upon the nature of Will, considered as the man in relation to action. When I say 'a new light,' I mean a light which is new as compared with the popular philosophy of the last generation. For that the most recent psychology is definitely corroborating the notions of Hellenic as of modern idealism, constitutes its absorbing interest, and its claim on the ethical student." (Preface.) The aim of the book is to indicate the chief lines along which the results of current psychological investigation afford this confirmation of philosophical and ethical idealism. The author's chief authorities in psychology are Mr. Bradley, Professor James, Mr. Stout and Professor Münsterberg. He has also learned much from Professors Ward and Sully. While his main interest is in volition, Mr. Bosanquet traverses rapidly the field of cognition and of feeling, and even devotes a lecture to the question of the relation of body and soul. Thoroughgoing in his philosophical idealism, he has no fear of scientific naturalism. "If you think the whole universe is mechanical or brute matter, then we can understand your trying to keep a little mystic shrine within the individual soul, which may be sacred from intrusion and different from everything else—a monad without windows. But if you are accustomed to take the whole as spiritual, and to find that the more you look at it as a whole the more spiritual it is, then you do not need to play these little tricks in order to get a last refuge for freedom by shutting out the universe. It has always been the most spiritual philosophy that has been most audacious in simply taking the soul as an operation or appearance within the universe, incapable of being cut off from other operations and appearances, and demanding to be investigated quite impartially with reference to the origin and connection of its elements" (pp. 9, 10). "The search for an innermost self, a sacred holy of holies in one's self which never changes and is never obtruded upon, is hopeless" (p. 89). All this may be true, but Mr. Bosanquet can hardly be said to have even contributed to its establishment in the present volume. The pace is too

rapid, the tone too jaunty, the atmosphere too reminiscent of the University Extension lecture-room in which the book originated. Indeed, if the truth must be spoken, what Mr. Bosanquet has given us is rather a collection of lecture-notes than a book. And, notwithstanding his evident desire to understand and to state correctly the findings of scientific psychology, his no less evident desire to reconcile these findings with the teachings of philosophical idealism, combined with the long habit of the metaphysical attitude, has often prevented his distinguishing with the required precision the psychological from the philosophical aspect of the question.

JAMES SETH.

CORNELL UNIVERSITY.

The Psycho-Physiology of the Moral Imperative. J. H. LEUBA.
Amer. Jour. of Psychol., July, 1897.

The author does not write from the standpoint of the descriptive psychologist, or from that of the speculative or historical ethical writer. He does not enter into the quest for the *summum bonum* or the criterion of conduct. He takes it for granted that such exists, pays to the moral imperative a high and lofty tribute, and then seeks to ascertain its psychological correlate. The thesis of the article is that "the moral imperative is the psychical correlate of a reflective, cerebro-spinal, ideomotor process, the efferent end of which is organized into motor tracks coördinated for a specific action." The *imperative* element of conscience is compared with the phenomena of insistent ideas and their physiological mechanism. The non-moral is, however, carefully distinguished from the moral insistent ideas. The *reflective* element is characterized by the fact that with the sense of moral obligation there is always felt an antagonism of associated processes of the reflex-arc type. There is a retinal rivalry of ideas and their incipient motor accompaniments, so to speak. In other words, there is deliberation or reflection. The antagonism of the motor coördinations is on the side of consciousness an antagonism of intentions or ends. The *ideo-motor* element is that which James has made familiar to all. Then, again, the moral imperative is, on the physical side, non-sympathetic, cerebro-spinal in its nature. The calm, passionless insistence of the moral dictate is due to its freedom from the affecto-emotional life of the vegetative life, to the absence of the stimuli and passions of the sympathetic nervous system. Thus the moral dictate is more or less free from the desires, cravings and feelings which are peculiarly self-ish or personal. Thus we obtain the autocratic, impersonal, pas-

sionless, universal, obligatory, insistent, categorical imperative—the Stern Daughter of the Voice of God.

The author's literary style is excellent. He has made a distinct contribution. He has placed the moral imperative as the psychic expression of a well-defined species of reflex-arc process, in its natural and legitimate place in the unified and uniformly acting psychic system of modern science. His suggestion is also good that, "if the 'endeavor' of the organism through its career of evolution is shown by comparative anatomy to have been towards the isolation of the sympathetic from the cerebro-spinal system, the greatest and most portentous *conscious* effort of the highest races during past milleniums has been to deliver the 'soul' from the influences of the 'body.'"

ARTHUR ALLIN.

UNIVERSITY OF COLORADO.

Evolution Ethics and Animal Psychology. E. P. EVANS. New York, D. Appleton and Co. 1898. Pp. 386.

This book is divided into two parts, of which only the second is of immediate interest to psychologists. It alone, therefore, will be taken account of in this review.

Mr. Evans pushes to extremes the view of animal mind held by Romanes. Even the actions of the pike who, after being prevented from devouring fish by the interposition of a glass plate, failed to dart at them when later it was removed, are attributed by him to 'docility and discrimination,' and 'judgment.' The glass plate did not merely inhibit an instinct; it inculcated 'a definite idea.' In general, Mr. Evans exhibits very well the common failure of this type of writers about animals to know just what they are looking for, and just what the facts which they do get mean. He repeats, with some interesting additions, the traditional anecdotes of marvellous performances by animals and the customary asseverations that reason is needed to account for them. But he has apparently made no effort to discover the workings of animals' minds in those circumstances where they are systematically given a chance to show what reasoning or mentality of any sort they possess. He has, that is, neglected the field of actual experiment and of observation of the actions of animals in the hands of professional trainers. This neglect is unfortunate. If the time-worn stories had had enough meaning to give us an animal psychology that scientists could agree about, we should have had one without Mr. Evans' book. Future writers on animal intelligence ought to realize that salvation does not come from such a source.

In saying that Mr. Evans does not know what his facts mean when he gets them, I refer not only to his wilful introduction of rational thinking whenever he can find the slightest pretext, but also to his perverse logic in general. He forgets, that is, that if a rational faculty be present in an individual, it should display itself more than once or twice a year. He forgets, also, that if an animal has mentality enough to solve a certain practical problem, it should also solve a great number of simpler and more obvious problems. If, for instance, the storks congregate purposely to try criminals, they ought also to congregate to commune about the best feeding-grounds. He forgets, finally, that if one individual of a species has a certain grade of mentality, and if variation within the species in other respects is slight, there should be many others in the species with like mentality. In fact, to the reviewer, it seems that about the only use to which these stories can be, at any rate have been put, is misuse.

The most useful part of Mr. Evans' book to the comparative psychologist is the bibliography, which is convenient and sufficiently complete.

EDWARD L. THORNDIKE.

COLUMBIA UNIVERSITY.

Affirmations. HAVELOCK ELLIS. London, Walter Scott. 1898. Pp. vii + 248.

A series of papers on 'Nietzsche,' 'Casanova,' 'Zola,' 'Huysmans,' 'St. Francis and Others.' The author says (Preface): "In this book I deal with questions of life as they are expressed in literature, or as they are suggested by literature. Throughout I am discussing morality as revealed or disguised by literature." The essays are clear, forcible and penetrating.

J. M. B.

NEW BOOKS.

Outlines of Descriptive Psychology. GEORGE TRUMBULL LADD. New York, Charles Scribner's Sons. 1898. Pp. xi + 428. \$1.50.

A Primer of Psychology. EDWARD BRADFORD TITCHENER. New York and London, The Macmillan Company. 1898. Pp. xvi + 314. \$1.00.

Outlines of Sociology. LESTER F. WARD. New York and London, The Macmillan Company. 1898. Pp. xii + 301. \$2.00.

Evolutional Ethics and Animal Psychology. E. P. EVANS. New York and London, D. Appleton & Co. 1898. Pp. 386.

- The Psychology of Suggestion.* BORIS SIDIS. With an Introduction by WILLIAM JAMES. New York, D. Appleton & Co. 1898. Pp. x + 386.
- Practical Ethics.* A Collection of Addresses and Essays. HENRY SIDGWICK. London, Swan & Sonnenschein; New York, The Macmillan Company. 1898. Pp. xvi + 260. \$1.50.
- Essai sur l'obligation morale.* GEORGE FULLIQUET. Paris, Alcan. 1898. Pp. 454.
- L'Absolu et sa loi constitutive.* CYRILLE BLONDEAU. Paris, Alcan. 1897. Pp. xxv + 344.
- La personne humaine.* L'ABBÉ C. PIAT. Paris, Alcan. 1897. Pp. 404.
- Le rationnel.* GASTON MILHAUD. Paris, Alcan. 1898. Pp. 179.
- La philosophie de Nietzsche.* HENRI LICHTENBERGER. Paris, Alcan. 1898. Pp. 186.
- Positiv Æsthetika.* PEKÁR KÁROLY. Budapest, Hornyánszky Viktor Könyvnyomdája. 1897. Pp. xiv + 672.
- Methodologische Beiträge zu psycho-physischen Messungen.* ARTHUR WRESCHNER. Leipzig, Johann Ambrosius Barth. 1898. Pp. vi + 238.

NOTES.

WE have received the 'Addresses and Proceedings' of the National Educational Association (Univ. of Chicago Press, 1897), which, as in former years, contains what is good, bad, and indifferent. Possibly the feature of most permanent interest in these voluminous documents will turn out to be the reports of the various committees on educational questions of interest. With these we may call attention to the very valuable 'Reports of the (National) Commissioner of Education' (1895-6, 2 vols.), and to the 'Bibliography of Education,' by W. S. Monroe (Appletons), which gives us for the first time a reliable index to literature in this wide-reaching department. Will 'pedagogy' finally 'be heard for its much speaking'?

J. M. B.

PSYCHOLOGISTS as well as students of philosophy will welcome the completion of the translation of Zeller's 'Philosophy of the Greeks' by the issue (Longmans) of the two volumes devoted to 'Aristotle and the earlier Peripatetics.' The chapters on the 'Physics'

(Psychology, Biology, etc.) of Aristotle occupy 300 pages, and constitute probably the best exposition extant. J. M. B.

AMONG the volumes announced in The Science Series, to be published by Messrs. G. P. Putnam's Sons and edited by Professor J. McKeen Cattell, are the following: 'The Groundwork of Science:' By Dr. ST. GEORGE MIVART. The 'History of Science:' By C. S. PIERCE. 'The Study of Man:' By Professor A. C. HADDON. 'General Ethnography:' By Professor DANIEL G. BRINTON. 'Recent Theories of Evolution:' By Professor J. MARK BALDWIN. 'Heredity:' By J. ARTHUR THOMPSON. 'Age, Growth, Sex and Death:' By Professor CHARLES S. MINOT.

THE first number of *L'Année sociologique*, edited by M. DURKHEIM, professor of sociology at Bordeaux, and published by Felix Alcan, Paris, will be issued early in the present year, covering the year 1897.

MR. SHADWORTH H. HODGSON, formerly President of the Aristotelian Society, has in the press of Messrs. Longmans, Green & Co. a new philosophical work entitled 'The Metaphysic of Experience.' It consists of four books, distributed over as many volumes. The titles will be as follows: Book I. General Analysis of Experience; Book II. Positive Science; Book III. Analysis of Conscious Action; Book IV. The Real Universe.

AN expedition under the direction of Professor A. C. Haddon will leave England at the beginning of March for Torres Straits and Borneo, where anthropological investigations will be undertaken. Drs. Rivers, McDougall and Myers, all of Cambridge, will accompany the expedition with a view of collecting data on the senses and mental processes of the natives.

INTERNATIONAL Congresses of Physiology and Zoölogy meet concurrently in Cambridge, England, during August of the present year. The programs of both Congresses will doubtless prove attractive to psychologists.

MR. GEORGE SANTAYANA has been appointed assistant professor in philosophy in Harvard University, and Dr. Robt. MacDougall, of Western Reserve University, instructor in philosophy and assistant director of the Psychological Laboratory.

DR. E. G. LANCASTER has been made professor of psychology and pedagogy at Colorado College.

DR. W. H. R. RIVERS, of St. John's College, Cambridge University, has been appointed lecturer in experimental psychology.

